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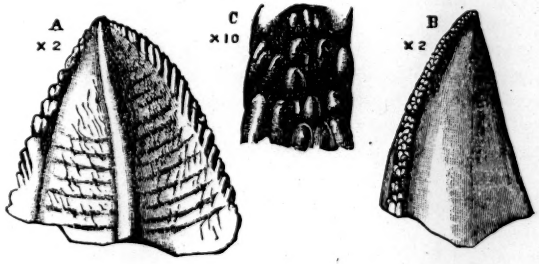
ORIGINAL ARTICLES.

I.—Note on an Iguanodont Tooth from the Lower Chalk ("Totternhoe Stone"), near Hitchin.

By E. T. Newton, F.G.S.

THROUGH the generosity of Mr. W. Hill, the Museum of Practical Geology possesses a tooth found in the "Totternhoe Stone" in the neighbourhood of Hitchin, which is of especial interest inasmuch as it is undoubtedly closely related to *Iguanodon*, and is of later date than any British Dinosaurian remains hitherto recorded; the *Acanthopholis horridus* of Prof. Huxley,[1] from the Chalk Marl, and the *Trachodon* (*Hadrosaurus*) *Cantabrigensis*, of Mr. Lydekker,[2] from the "Cambridge Greensand," being, up to the present time, the most recent of known British Dinosaurs. Remains of allied forms, however, have been found at a higher horizon of the Chalk of Maestricht, and have been described by Prof. Seeley,[3] and M. L. Dollo.[4]



Explanation of Figures.

A. Iguanodont tooth from the "Totternhoe Stone" near Hitchin. Enamelled surface. Enlarged twice nat. size. B. Same tooth, seen in profile.
C. Portion of crenulated edge. Enlarged ten times nat. size.

The specimen to which attention is now directed is the upper part of the crown of an unworn tooth, in an admirable state of preservation; its lower part was evidently broken away before it was imbedded in the Chalk, and the sharpness of the fractured edges, as well as of the denticulations, shows that it has under-

[1] Geol. Mag. Vol. IV. 1867, p. 65.
[2] Q.J.G.S. vol. xliv. p. 47.
[3] Q.J.G. S. vol. xxxix. p. 246.
[4] Bull. Mus. Roy. Hist. Nat. Belg. vol. ii. p. 205, 1883.

gone no subsequent attrition. The greatest length of the portion preserved is about 0·6 inch, and its width from before backwards 0·5 inch, whilst the greatest thickness, at the fractured end, is about 0·4 inch. The enamelled surface is somewhat pointed at the apex, and has a single crest, extending from top to bottom, which divides this surface into two unequal areas. The anterior and posterior margins are ornamented with strong serrations, from which grooves extend, almost vertically, some distance on to the enamelled surface. When viewed from before, the crown is wedge-like, and the serrations of the edge, when seen with a lens, are found not to be simple, but each of them is itself denticulated, giving a mammillated appearance to the somewhat thickened edge. The enamel is slightly roughened by small vermiform rugæ.

The close resemblance between this tooth, so far as preserved, and some of those figured by Sir R. Owen[1] as *Iguanodon Mantelli*, would lead to the inference that it belonged to the same species were it not that one of these (figs. 15, 16) having only a single ridge on the enamelled surface, has more recently been referred by Prof. Seeley[2] and Mr. Lydekker[3] to *Trachodon* (*Hadrosaurus*), and as our specimen has only a single ridge, it would seem, at first sight, that it also should be referred to *Trachodon*. However, Sir R. Owen's figures 10 and 17 have only small secondary ridges of the enamel, and these do not extend far from the base of the tooth, our specimen therefore might well be a portion of such a tooth with the small ridges broken away. Besides this, another tooth with only a single enamel ridge, which is from the Wealden, is called *Iguanodon* by Sir R. Owen,[4] and no one seems to have questioned the correctness of this determination.

Then, again, with regard to the serrations, these are coarser than in the figures of *Trachodon*,[5] and the grooves extend from them some distance over the enamelled surface, a character seemingly wanting in *Trachodon*. The mammillations of the serrations find their counterpart in both *Iguanodon Mantelli* and *Trachodon* (*Hadrosaurus*) *Foulkei*.

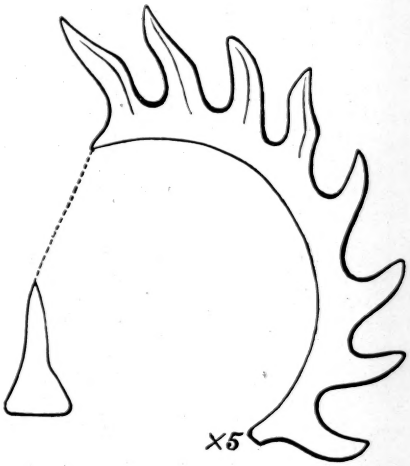
The Dinosaurian remains from the Maestricht Chalk described by Prof. Seeley and M. L. Dollo include certain bones which are said to have affinities with *Iguanodon* and *Hadrosaurus*; they have been named *Orthomerus Dolloi*. It is possible that the tooth from the "Totternhoe Stone" may prove to belong to the last-named genus; but as there is no evidence at present of the teeth of *Orthomerus*, it would scarcely be wise to refer our specimen to it; on the other hand, it is quite certain, that in whatever genus this tooth may eventually have to be placed, it is very closely related to *Iguanodon*, and with this it will provisionally be included, and named specifically after its donor *Iguanodon Hillii*.

[1] Pal. Soc. Cret. Rept. suppl. ii. pl. vii. figs 10, 15, 16, 17.
[2] Q.J.G.S. vol. xxxv. p. 591, 1879. [3] Q.J.G.S. vol. xliv. p. 47, 1888.
[4] Pal. Soc. Rept. Wealden, pt. ii. pl. xviii. fig. 6, 1855.
[5] Leidy, Smithsonian Contributions, vol. xiv. 1865.

II.—Note on a New Species of *Onychodus* from the Lower Old Red Sandstone of Forfar.

By E. T. Newton, F.G.S.

THE occurrence in the Ledbury "Passage-beds" of a united series of teeth referable to the American type of fossil fishes named by Prof. Newberry *Onychodus*, was made known by Mr. A. Smith Woodward in the Geological Magazine for November, 1888; and in the British Museum Catalogue in 1891. Since then I have met with another example of the genus among the stores of the Geological Survey, which, as it seems to be quite distinct from the Herefordshire species, and is from a distant locality, deserves to be placed on record.



Onychodus scoticus, n. sp., from the Old Red Sandstone of Forfar. Presented to the Museum of Practical Geology by James Powrie, Esq., F.G.S., of Reswallie.

The specimen consists of a series of teeth from the Lower Old Red Sandstone of Turin Hill (?), Forfar, and has been presented to the Museum of Practical Geology by Mr. James Powrie, of Reswallie, who has done so much to extend our knowledge of the fauna of these rocks. This series of teeth, is preserved in a shaly layer covering a small block of hard grey sandstone; and the shale having been split open, the fossil itself has been broken through from end to end, one half being retained in each piece of shale; portions of some of the teeth, however, have fallen out. There are eight teeth placed one before another in a single row, with their bases coössified so as to form a rigid semicircular base, which measures about 9 mm. in a straight line from end to end. Two of the teeth seem to have been quite perfect when imbedded in the shale; but the others are more or less worn down and rounded. The longest and most perfect tooth is about 4 mm. in length; and the shortest nearly 3 mm. The basal portion, so far as one can judge from a

wax impression, is about 2 mm. wide. Each unworn tooth is strongly curved, or rather crooked, with an acute somewhat recurved point. The concavity of each tooth is directed towards the end where they are least worn. The cavities, left by the teeth which have fallen out, show that these teeth were provided with a very distinct ridge on each side, and they appear to have been flattened from before backwards.

An examination of the broken surfaces with a lens reveals a very vascular condition of the base, and large vessels pass from this into each tooth. No division can be traced between the bases of the teeth, which are evidently firmly united to each other.

Compared with *Onychodus anglicus* and *O. arcticus*, the present specimen differs in the more uniform size of its teeth, the longest of which is not half the length of the most perfect tooth in the former species, from which it also differs in the greater curve, and non-involution of the base; and further, its teeth have distinct lateral ridges and no central cavity.

The uniform size of the teeth in the Forfar specimen agrees better with the American forms, but they are much smaller than in any one of the three species described by Prof. Newberry, and are of a different shape. In *O. Ortoni* the teeth are said to be sunk in the base "as posts are planted in the ground."

British examples of this genus are so rare that even such fragments as that above described may well receive specific distinction, and I propose to name it *Onychodus scoticus*.

The following are the species of *Onychodus* now known:—

O. sigmoides, Newberry. Corniferous Limestone (Lower Devonian), Ohio. Monog. U. S. Geol. Surv. vol. xvi. p. 56, 1889.
O. Hopkinsi, Newberry. Chemung Group (Upper Devonian), Delaware Co., New York. *Ibid.* p. 99.
O. Ortoni, Newberry. Huron Shale (Upper Devonian), Franklin Co., Ohio. *Ibid.* p. 71.
O. anglicus, Smith Woodward. Lower Old Red Sandstone Passage Bed, Ledbury, Herefordshire. Geol. Mag. Vol. V. 1888, p. 500, and Cat. Foss. Fishes, Brit. Mus. part 1, p. 393, 1891.
O. arcticus, Smith Woodward. Lower Devonian, Spitzbergen. Geol. Mag. Vol. VI. 1889, p. 499; and Ann. Mag. Nat. Hist. vol. viii. 1891, p. 1.
O. scoticus, Newton. Old Red Sandstone, Forfar.

III.—Some New Forms of Hyaline Foraminifera from the Gault.

By Frederick Chapman.

(PLATE II.)

In the course of investigations amongst the Microzoa from the Gault of Copt Point, Folkestone, some Foraminifera came under my notice, which are hyaline representatives of the usually arenaceous type *Webbina*. These forms are remarkably similar in external appearance to those of the arenaceous group, but microscopical examination of the test under a high power reveals the finely tubulated structure characteristic of the hyaline type.

It must, however, be borne in mind that the isomorphous genera in the three groups of the Foraminifera based upon shell-structure

are of classificatory rather than biological value: it being most probable that the same organism may possess the ability to form the structure of its external covering according to the conditions under which the Rhizopod exists.

Dr. Sollas has described[1] two forms of Hyaline Foraminifera from the Cambridge Greensand under the generic term *Webbina*, which presents the same finely perforated structure as the Gault specimens, and he has proposed to reserve the generic term *Webbina* for the type he describes possessing the perforate character. The structure of the shell wall of these perforate varieties, as Dr. Sollas points out, shows the type to be closely related to the Rotaline Foraminifera: and since there is present a shelly flange which may possibly be equivalent to the keel-like edge of some *Pulvinulinæ*, I would therefore suggest that the perforate varieties of *Webbina* might form a separate genus, which would find a place between *Rupertia* and *Pulvinulina*.

Another somewhat remarkable form found is interesting on account of the prominence which it gives to the fact of the great variability of the Foraminifera. This is an adherent and ramifying variety of *Polymorphina*. In general appearance it somewhat resembles the form *Sagenella* of Dr. Brady, especially in the shape of the terminations with their simple apertures. In a few specimens, however, the portion of the test, which is rounded and inflated, and is much smaller than the ramified portion, shows a decidedly chambered structure, identical with that of *Polymorphina*; and this is confirmed by one individual of which the test was broken, showing the chambered portion. The cervicorn and fistulose varieties of *Polymorphina* are very fully discussed in Messrs. Parker, Jones, and Brady's paper on the genus *Polymorphina*;[2] under the name of *Polymorphina Orbignii*, Zborzewski, sp.

Vitriwebbina, gen. nov. Pl. II.

General characters.—Test opaque to translucent, of a whitish or pale-brown colour. Shell-wall very finely perforated. Consisting of a single hemispherical or pyriform chamber, or of a graduated series disposed usually in a curved line, and adherent upon some foreign substance. The chambers are connected by stolon tubes, very distinctly seen on the under surfaces of the specimens which have become detached. The surface of the shell may be smooth, pitted, or, as in Dr. Sollas's specimen, tuberculate.

Vitriwebbina Sollasi, sp. nov. Pl. II. Figs. 1–3.

This form consists usually of one, but sometimes of even four pyriform chambers, always adherent, in many cases to rolled fragments of phosphatic nodules. The test is usually white, though sometimes pale-brown and opaque, and the shell-wall is finely tubulated, with the exception of a thin encircling flange of shell material as it were completing the fusion of the shell to its attachment. This peculiar flange of shell substance is present in Dr.

[1] Geol. Mag. Dec. II. Vol. IV. 1877, p. 102–105, Pl. VI.
[2] Trans. Linn. Soc. vol. xxvii. 1870 (1871), pl. xlii. figs. 38 *a*—*o*, pp. 244–248.

Sollas's specimens, but turned *inwards* on the foreign body. This variety occurs in Price's Zones I. III. V. VII. and XI. at 20 feet from the top of the Gault, at Copt Point. I have also since found it in the Gault at Battlebridge, Merstham, Surrey. I have associated the name of Dr. W. J. Sollas with this form, who first described the perforate type.

Vitriwebbina lævis, Sollas, sp. Pl. II. Fig. 4.

This form has already been recorded by Dr. Sollas, and is mentioned here in passing as occurring in the Gault beds of Folkestone in Zones IV. VII. and XI. at 50 feet from the top.

Polymorphina Orbignii, Zbor. sp., *cervicornis* var. nov. Figs. 5 and 6.

The test, which is generally found attached to shell fragments, such as *Nucula*, commences with a polymorphine arrangement of chambers, which proceeds to take on a wild flattened fistulose growth, sometimes six times the length of the initial series of chambers. The apertures are simple orifices at the terminations of the branchlets. An example occurs in which the branching growth has just commenced. This variety is found in Zones III. and VII. of the Gault at Copt Point.

In concluding these notes, I wish to acknowledge the kind advice I have received from Prof. T. Rupert Jones, F.R.S., and also the valuable assistance of my friend Mr. C. D. Sherborn, F.G.S.

EXPLANATION OF PLATE II.

FIG. 1.—*Vitriwebbina Sollasi*, sp. nov., showing thin encircling flange *f* (× 60).
,, 2.—*V. Sollasi*, fragment of test showing tubulate structure (× 360).
,, 3.—A specimen of ditto with four chambers (× 60).
,, 4.—*V. lævis*, Sollas, sp. (× 40).
,, 5.—*Polymorphina Orbignii*, Zbor. sp., var. *cervicornis*, attached to a fragment of shell of *Nucula*. The lines indicating the later growth are slight grooves in the shell to which the Foraminifer was attached, as if the foreign body were partially dissolved along the lines of attachment; *p.* represents the polymorphine series of chambers. The whole of the shell contains an infilling of pyrites (× 30).
,, 6.—The apex of one of the branches of *P. Orbignii*, var. *cervicornis*, showing the finely tubulated shell-wall.

IV.—The Absence of Glacial Phenomena in large Parts of Western Asia and Eastern Europe, etc.

By Henry H. Howorth, M.P., F.G.S., etc.

IN some papers which you have done me the favour to print in the Geological Magazine, I have endeavoured to apply a new touchstone to test the age of high mountain chains and of land of high elevation, namely, the presence or absence of distinct and prominent traces of former glaciation, and I have argued that where such traces are not forthcoming in a very unmistakable manner, we are justified in concluding that these highlands have been elevated since the so-called Glacial Period. I have endeavoured to apply the touchstone in question to the Ural and Altai Mountains, to the Thian Shan and Himalaya ranges in Asia, and to the great Cordillera which binds together the two continents forming the New

World. I wish to make my survey more complete by an examination of an interesting area comprising Eastern Europe and South Western Asia.

Before addressing myself directly to this issue, however, I feel under an obligation to reply to the criticisms of Mr. Blanford. They appeared when I was far away from England, or they should have had an earlier notice.

In one of my papers I quoted the testimony of four geologists of wide-spread fame and great experience, keen and patient observers, whose works are geological classics, who had visited and explored with great pains the Urals, the Altai Mountains and the Thian Shan range, namely, Humboldt, Tchihatchef, Von Cotta, and Severtsof. All these observers had examined geological phenomena in many latitudes and were familiar with glacial marks, and all of them failed to find the footmark of the Glacial Period in the ranges in question, although they looked for them. This is assuredly a strong case. How does Mr. Blanford dispose of it? He says he can attach no value to the evidence brought forward, because (mark the phrase) "I myself was at one time led away by it." To measure the capacity of some of the greatest geologists of modern times for distinguishing what I venture to think are among the most obvious of phenomena, by the fact that the critic, like every other good man, has once made a mistake, seems to me to involve an appeal to other than scientific reasoning.

Let me add that, as Mr. Seebohm has reminded me, Père David, who spent many months at Moupin, on the frontier of China and Tibet, at the foot of a mountain as high as Mont Blanc, failed to find traces of old glacial action there, a result which was, I believe, also reached by Richthofen in other parts of China.

This is assuredly a very important addition to the strong chain of witnesses I have previously quoted in your pages in references to the great congeries of mountains in Eastern Asia.

Turning to the Himalayas, Mr. Blanford does not dispute the fact that in Peninsular India and in the great plains of the Indus and the Ganges, the real Hindostan, no true glacial phenomena are forthcoming. He urges, however, that they do occur high up in the Himalayas on a scale quite incommensurate with my arguments. I have not visited the Himalayas myself, but I did quote two experienced observers, Mr. Campbell and General McMahon, whose testimony is decidedly at issue with Mr. Blanford, and who had visited the country. It is true that some of Mr. Campbell's *theories* are fanciful enough; but I never met any one who disputed his keen eye for, and his extraordinary knowledge of, glacial facts in all parts of the world. Apart from these explorers, however, I appealed to a witness who is unbiassed by our discussions, who has no views whatever as to the glacial theory. I mean the Sun. Mr. Blanford seems to have a contempt for photographs. I am bound to say I differ from him *toto cœlo*; and I have examined glacial phenomena over as much ground as most people.

In regard to the main and most satisfactory evidence of glaciation,

more satisfactory than either boulders or scratched stones or moraines, namely, the rounded and curved outlines always present when a glacier has polished a valley, I know of no testimony so excellent as that of photographs, for by their help we can rid ourselves at once of the personal equation of the observer whether he have ice or water on the brain. I appealed to photographs—many of which I had seen—and to that appeal I adhere. I contend they are a complete justification of my views.

I never argued that when you get up into the higher valleys of the Himalayas there is no evidence of the former presence of glaciers. I emphatically stated that there is, as Hooker and others showed long ago; but I did and do contend that these traces of former glaciers are infinitesimal compared with what they ought to be if the Himalayas had existed when the great Rhone glacier was depositing its famous loads far and wide.

I do not quite understand the reference to a difference of latitude which Mr. Blanford says I have overlooked. What has latitude to do with the question? Assuredly the Urals, which rise in places to a height of 1525 metres, and which are covered with snow for eight months in the year, the Altai Mountains, which are higher, and the Northern Rockies are all in latitudes which, if that had anything to do with the question, would have placed them at least on the same level as the Alps, the Pyrenees, and the Scotch Mountains, which have such abundant traces of glacial action upon them. It is not latitude that has to do with the question, but an abundant supply of moisture, and a sufficiently powerful source of cold. With these two conditions there will be abundant snow and ice, whatever the latitude.

When I spoke of a great Asiatic Mediterranean having existed during the Glacial period, it was not, as Mr. Blanford seems to suppose, to invoke an army of icebergs as having existed there, which I altogether disbelieve in; but to point out that this reservoir of water close at hand would supply the very moisture necessary for a tremendous glaciation of the Asiatic mountain chains, and notably of the Altai, if those mountain chains had then existed.

Granting this supply of moisture, granting the capacity of these high ranges as excellent condensers, we have in the wide area they cover the necessary elements for a huge development of ice. If there is no evidence of this ice having existed, then I would urge again that it goes a long way to prove that the great mountains did not exist either at the time in question.

That the glaciers have been bigger no one disputes, but I hold with Godwin-Austen that this was comparatively recently. On this subject he wrote as follows in the Geological Journal many years ago:—

"I have often been struck by the indications of considerable amounts of change of temperature *within what may be called our own times*. The proofs of this are to be found in many parts of the great Himalayan chain. These consist in the numerous terminal moraines which in so many places abut on the larger rivers, down to which

point glaciers must have once descended, and which in some cases must have rivalled in length the present ones of the Mustakh range. . . . . Among the proofs that *there has been a change of temperature of recent date* are the following. Many passes which were used even in the time of Rajah Ahmed Shah of Skardo, are now closed. The road to Yarkand over the Baltoro glacier, which before his time was known as the Mustakh, has by the increase of the ice near the pass become quite impracticable. The men of the Braldoh Valley were accordingly ordered to search for another route, which they found in the present pass, at the head of the Punmah glacier above Chiring. Again, the Jussèspo La can now be crossed only on foot; whereas in former times ponies could be taken over it. The pass at the head of the Hoh Loombah is now never used, though there is a tradition that it was once a pass; no one, however, of the present generation that I could hear of had ever crossed it. Certain large glaciers have advanced, such as that at Arundu, of which the old men assured me that in their young days the terminal cliff was $1\frac{1}{2}$ miles distant from the village. Mr. Vigne says, 'it was a considerable distance,' it is now only 400 yards. A like increase has taken place at Punmah, where, within the last six years, the road has been completely covered by the ice and moraine, and where Mahomed, my guide, told me the old camping ground was, now lies a quarter of a mile under the ice; the overthrown trees and bushes plainly testified to the recent advance which this mass has made; this evidence was equally well seen along the side of the Arundu glacier. Even so lately as twelve years since, the people of Shigar were enabled to get two crops off their fields; thus the first crop (barley) was followed as soon as cut by a second (kungŭni), which ripened by the end of autumn. Since that time it will not come to maturity, so that after the barley the fields now lie fallow, and the kungŭni has now to be sown earlier in the season" (Journ. Roy. Geog. Soc. vol. xxxiv. p. 51.)

In regard to the shrinkage of the glaciers, evidence is not of the same kind, for the good reason that during recent years the stage has been one of growth, but the same author's descriptions point to the traces of former extension as being comparatively recent. Thus he says of the Punmah glacier: "This glacier has in some past years been upwards of a 100 feet thicker than it now is, as shown by its lateral moraines, and the grooved and scratched rocks on either side" (*id.* 30). Again, speaking of the glacier of Biafo, he says: "The present thickness of the ice is a point not easily determined; but, judging from striæ in the sides of ravines from which glaciers have retired, from 300 to 400 feet, is not an exaggerated allowance for what they once have been" (*id.* 50). Speaking of Basho he says: "Glacier action of former times was here very apparent in the great masses of angular rocks above the village. The enormous collection of angular fragments in the terminal moraine of a large glacier, the remains of which are to be sought higher up, and where now it is only 4 or 5 miles long, with broad feeders from the mountains on the west side" (*id.* 54). This will suffice from Godwin-Austen.

I might have quoted another witness. In a note to an admirable and most interesting work on Eastern Persia well known to me and to Mr. Blanford, and on page 470, I find the following: "My brother, Mr. H. F. Blanford, has suggested to me that the greater humidity of Persia and the neighbouring countries in former times may have partly accounted for the former great extension of glaciers in the North-West Himalayas. If the west wind so prevalent in North-Western India were moist, instead of being hot and dry, as it now is, there would be certainly a great increase in the deposition of snow on the Western Himalayan ranges." Nay, I might have, as I have before, quoted Mr. Blanford against himself; for, in a paper on Persian superficial deposits, he argues that the drying up of Central Asia is connected with the elevation of the Steppe region of Central Asia (J.R.G.S. vol. xxix. p. 500).

In this I completely agree. The desiccation of Central Asia is going on at this moment. We have a great deal of evidence about the shrinkage of its lakes and the disappearance of its streams in historic times. With this desiccation I hold the shrinkage of the Asiatic glaciers has also proceeded; and we need not, especially if we are champions of Uniformity, go back beyond a reasonable date in order to find an ample and sufficient cause for the increased length of the Himalayan glaciers in recent times.

Another point in Mr. Blanford's criticism I do not quite follow. I understand him to say that because the Wild Ass and the Antelope can live now at great heights, the Rhinoceros could do the same. The Rhinoceros is essentially a tree-feeding and shrub-feeding animal, and does not graze on short grass; and we know the kind of trees which the *Rhinoceros antiquitatis* fed upon. To postulate that the Rhinoceros could live where the Antelope lives is to me, like saying that the Zebra could live where the Wild Ass of Mongolia lives; that Cape Buffalo could live where the Bactrian Camel lives; and that the Bison could exist where the Musk Sheep thrives. I do not understand the argument: nor do I understand why the existence of a zoological sub-province in China, which has been established by a chain of observers from Brian Hodgson to Père David, precludes the notion, otherwise so strongly supported, that the Highlands of Eastern Asia are a recent feature in physical geography.

This, I think, completes my reply to Mr. Blanford; and I will now pass on. My argument was not meant to be restricted to the great masses of mountains in Eastern Asia. These masses of mountain are closely united in their physical history with the highlands stretching from the Hinduh Koh westward through Persia; and it is a remarkable fact that there also we have a singular absence of erratic phenomena and of traces of a so-called Glacial period.

On this subject Mr. W. T. Blanford says: "In Persia the country, although greatly elevated above the sea-level, is covered with drift; but I found no signs of striation on the pebbles," nor had he been able to detect glacial markings on extensive plateaux more than

6000 feet above the sea, with peaks rising to 12,000 feet and even more (Q.J.G.S. vol. xxx. p. 478). Elsewhere he says: "Of glacial action in Persia there is, perhaps, a trace in the thick gravel found locally, as near Karman, on ranges of considerable height. *At the same time no clear evidence of ice action could be detected. In the Elburz Mountains, which are in about 36° latitude, neither Dr. Filippi nor I could find any evidence of former glacial action.* It is true that neither of us had much opportunity for exploring; but it is remarkable that Abich should have called attention to the same absence of glaciation in the Caucasus" (Blanford's Eastern Persia, p. 470).

Dr. Filippi, whose memoir is before me, speaks in the same terms. He very naturally asks where the great mass of water can have come from to spread the gravel which occupies so much of the surface of Persia, *to explain which he says we must not have recourse to a glacial epoch of which there is no trace in the Elburz Mountains,* "di cui non vha nelle montagne dell' Elburz alcuna traccia" (Atte della Soc. It. de Sci. Nat. vol. vii. p. 283). The same writer calls attention to another fact which I would quote here, and which corresponds with what I have said of the Ural Mountains and the American Cordillera, namely, that these great masses of high land form no zoological frontier, and are therefore presumably of very recent origin. "A great continuous barrier," he says, "like this, ought, like the other principal mountain chains of the world, to form a frontier separating two sensibly distinct faunas, but this is not the case. There is a greater difference between the fauna on the two sides of the Alps, and on the east and west of Europe than on the two sides of the Elburz" (*id.* 279–280). He further urges that the fauna of the high ground in Western Persia is essentially that of the Caspian depression and of the Turanian Steppes, which seems to me to also point to these highlands having been elevated very recently.

No doubt Mr. Palgrave, who by the way was not a geologist, did profess to find considerable traces of old glacial action in the neighbourhood of Erzerum, as others have professed to find them in the Caucasus. In answer I would refer to the observations of a most acute and experienced geologist, namely Abich.

In an elaborate memoir by him on the geology of the Caucasus and the mountains of Armenia and North Persia, published in the 7th volume of the Memoirs of St. Petersburg Academy, he says: "The distribution of erratic blocks, together with the associated phenomena of the grinding and polishing of rocks, is foreign to the Caucasus. Nevertheless, large blocks of stone and masses of rock of large dimensions, like erratic blocks in many respects occur in some of the valleys, especially those of the Terek; often too they have travelled some distance. But the transport of these blocks has nothing in common with the true diluvial phenomena of the period of erratic blocks of the European mountains, but is assignable merely to alluvial action which still operates, though in a diminished manner." He describes how, through the conformation of the valleys, especially in such places as the narrow pass of Dariel, great barriers of debris are found which dam back lakes 200 or 300 feet deep, and

when these burst their bounds, great masses of stone are carried along the Terek Valley as far as Wladikawkas. Abich mentions how the great earthquake in 1840 which destroyed the village of Arguri in the district of Ararat, was followed by a great rush of waters which bore along great masses of rock much larger than those now to be seen in the Terek Valley. Similarly, he says, great blocks of 250 to 300 feet in circumference have been carried from the district of Ararat for a distance of seven versts over an inconsiderable slope.

Abich goes on at some length to show that the moraines and other glacial phenomena of the Caucasus are very local and confined to the upper valleys, and do not protrude out into the open country in the way they do in the West of Europe, and that they are the obvious handiwork of the existing glaciers, and do not point to a glacial period. I prefer to quote his conclusion in his own words. "So läge denn in diesen Moränen des Gyoal Don, die einzigen von so bestimmten Charakter und solche Grösse mit in Kaukasus bekannt gewordenen, ein annäherndes Maass für das Maximum des Gletscherwerkungen von, wie sie seit dem Begunt und dem Verlaufe der alluvialzeit bis zur Gegenwart, hier durch lokale mit der Entstehung des Kasbek zusammenhängende physikalische Configuration der Kamm region bedingt worden, niemals aber das Privilegium einer besonderen etwa eine allgemeine erhöhete Gletscherbildung bedingenden, oder auch nur begünstigenden Epoche für das Kaukasus gewesen sein können" (Mems. St. Pet. Acad. vol. vii. pp. 519–523).

The same conclusions were arrived at by Mr. J. F. Campbell, whose wonderful appreciation of glacial phenomena is specially apostrophized by Professor Judd. As he neared the Kaspes peak, he says, "We drove over undulating plains of clay and passed a lot of large stones; but I could find no scratches. In the evening I walked to the right bank of the river and found a great ridge of clay which I took for a moraine; but even here I could find no scratched stones. I believe it to be part of a delta, I sketched and inspected brick-pits, and reluctantly gave up my Caucasian glacial hypothesis. . . . . We drove up a beautiful gorge with well-marked terraces of rolled stones at the mouth of it, and with many very large stones scattered about; but I saw nothing glacial in the gorge." Speaking of the route between Kazbeg and Tiflis, he says: "The outlines of the mountains are due to weathering. Except large stones I could find no trace of glacial action in the whole journey of 202 versts (133½ miles) . . . . After a careful search in the valley lower down than Zalkan all the large stones that I could find were smooth water-worn pebbles taken out of the clay and out of great beds of rolled stones which there make large hills. . . . . If ever glaciers worked in the range, their traces have been almost entirely obliterated. . . . . I could find no signs of glaciers even on the remnants of the old surface through which the water has dug a couple of thousand feet or more." Speaking of the only lake in Daghestan, the water of which is dammed by a dam of angular gravel, which may be a terminal moraine, he says: "I could find no

glaciated rocks anywhere about the lake. The moraine is the only mark of glaciation that I could identify in the whole range while travelling from end to end. . . . . From lat. 40° to 45° N. long. 50° to 35° E. I could not find one rounded hill or hollow, one scratched rock or stone, one perched block, one lake-basin, certainly due to glacial erosion, a glacier, or the trace of one. The highest hills are jagged sierras, the lower hills pyramidal or scarped, the valleys of all sizes are shaped liked a V" (Q.J.G.S. vol. xxx. pp. 460–466).

Let us now turn to another mountain-range, namely, that of the Lebanon in Northern Syria. Here Sir J. Hooker many years ago (Nat. Hist. Rev. Journ. 1862, p. 11) described the Cedars as growing on very considerable moraines, and this fact has been quoted in almost every manual of geology as evidencing the former glaciation of the Lebanon. The facts are, however, very doubtful indeed, and it is clear that in view of recent explorations they will have to be revised.

In a paper by M. Louis Lartet published in the 22nd volume of the Bulletin of the French Geological Society, embodying his researches extending over several months in Syria and Arabia, he refers to these supposed moraines, and urges that they are not really moraines and do not belong to the so-called Ice age at all, in the first place because he had never seen any scratched pebbles or other traces of glacial action in the midst of these deposits, and secondly, because they contain no basaltic pebbles or boulders showing that they must be older than the outbreak of basalt, etc., which have left such marks on the country, and he identifies them with the calcareous conglomerates long ago described by Botta (Mém. de la Soc. géol. de France, 158) to similar beds described by Russeger in the Orontes Valley, also composed of conglomerates cemented together by calcareous matter and which he treated as diluvian. Damascus is built on a similar bed, and it also occurs at the foot of Anti Libanus and on the eastern shores of the Lake of Tiberias (*op. cit.* p. 458).

There is a similar difficulty about the supposed glacial beds in the Atlas range. Ch. Grad, who explored the range, found no traces of ice-action there (see Zeitschrift der öst Gesellschaft für Meteorologie, 1873, p. 32).

If we turn to Asia Minor, we shall naturally turn for geological information to the detailed and masterly work by Tchihatchef. He explored the peninsula with great pains, and he says emphatically that all the phenomena of the Glacial epoch are absent from Asia Minor, and he adds that this is very curious, since the climate of Asia Minor is even under present conditions considerably influenced by the cold of Russia (Tchihatchef, Asia Mineure, 4th part, Geology, part ii. p. 485).

Crossing the Bosphorus, we have the same testimony. Boué, in his great work on European Turkey, says distinctly that the phenomenon of erratic blocks is foreign to the two Turkeys, *i.e.* Asia Minor and Turkey in Europe, as it is in all the south-east of Europe (La Turquie d'Europe, vol. i. p. 395).

In an elaborate memoir on the geology of European Turkey, published in 1876, in the 20th volume of the Austrian Geological Society, F. Von Hochstetter quotes Viquesnel for the statement that neither in the Izker valley (vol. ii. p. 373) nor in that of the Rielska Reka above Rilo Selo (vol. ii. p. 374), nor in the upper Mesta valley (vol. ii. p. 366), do these deposits bear any resemblance to moraines. Hochstetter says, "I can confirm this view of Viquesnel," and he goes on to show how easy it is sometimes to mistake a great mass of débris, the result of an avalanche, for a moraine, as in the case of a mass of granite blocks 10 mètres high in the valley of Rielska Reka, and he concludes, "Der Rilo, das höchste Gebirge der östlichen Türkei, hat ebenso wenig eine Getscherperiode gehabt, als der Balkan" (*op. cit.* pp. 460–461).

Neumayr is equally emphatic, and I will quote a passage from his well-known Erdgeschichte, published in 1887:—"No unmistakable traces of glaciation have as yet occurred in the Balkan Peninsula where they quite fail, except in the fact of the occurrence of some mountain lakes which may point to glacier action in the Kilo Mountains in South-Western Bulgaria. I myself have explored several of the high districts in Greece, Thessaly, and Macedonia, and neither on the Shar-dagh, near Uzkub, nor on Mount Athos, nor on Olympus, nor in the Ætolian Alps, nor in the Korax Mountains, have I found any traces which can be attributed to the work of glaciers (*op. cit.* pp. 598–599).

Moving northwards we have, in conclusion, to refer to the Carpathians. Traces of old glacial action have been diligently sought in this range, and more than one writer has described their existence; but they seem to be very local, if not doubtful. If they had been glaciated on a considerable scale, assuredly débris from their summits ought to be found dispersed over North Germany, which is literally covered with erratics that have come all the way from Finland and Scandinavia, where the mountains are for the most part very little, if any, higher.

Neumayr says that traces of glacier action of any importance are only to be found in the upper Tatra, that mass of serrated granite heights which extends between the districts of Zips and Leptan in Northern Hungary and Galicia. Long ago Zeuschner found traces of the moraines of an ancient glacier at Zakspan in the Tatra group, and in later times similar traces have been found on the south side" (*op. cit.* pp. 597–598). Neumayr gives a good illustration of these mountains, in which I confess I can see no traces of the ice plane in the rugged angular masses of granite, and I very much doubt the character of these so-called moraines which are so obviously inconsistent with the rough rocks above them. I notice also that Neumayr points out as the most striking evidence of old glacial action in the Carpathians the presence there of many mountain lakes, which, following in the footsteps of Ramsay, he attributes to glacial causes, a view in which a large number of geologists cannot share.

Turning to other parts of the Carpathians, Neumayr says quite

frankly that though traces of glacial action are not entirely absent, yet they are insignificant (anbedeutend). In the Liptan Mountains, he says, are some uncertain traces; but he himself had failed to find any evidence over a considerable stretch of the Central Carpathians. Paul and Tieze had, however, noticed some moraines on the Cherna hora, the highest part of the Eastern Carpathians, 2007 metres high, whence the Theiss and White Pruth spring (*id.* p. 599).

It seems to me that the evidence forthcoming to show that the Carpathians partook in the general glacial phenomena which have left such important and unmistakable traces in the much inferior ranges further west, such as the Vosges, the Morvan, etc., is quite unsatisfactory and insufficient, and that the problem should be again examined on the spot by some inquirers who do not mistake every heap of rolled stones for a moraine. It is incredible to me, if the Carpathians had existed in the Glacial age at the time when the mountains of Scotland and Ireland and Cumberland were loaded with ice, that we should have to search so minutely over them for any real traces of ice action, and to be actually limited to finding them on two or three of their higher peaks. The view that the further we go east in Europe, the smaller do the traces of glacial action become, had already occurred to others.

Penck says that Peschel was the first to observe (Volkerkunde, 1877, p. 43) that traces of glacial phenomena diminish in Europe as we go East. Of the three South German mountain ranges the Vosges present the greatest traces of glaciation, while in the Alps the intensity of the phenomena diminishes as we go East, and the Western Alps must have been more thickly covered with ice than the Eastern. The same is the case in America; only that there the intensity of glaciation diminishes as we go west. While the low-lying lands in the east of that continent were covered with ice, the mountain region on the west coast only harboured local glaciers (Penck, Vergletscherung der Deutschen Alpen, p. 438).

The evidence seems to me to point to the Carpathians being a very recent feature in European physical geography, and, as in the case of the Balkans, the Taurus, in Asia Minor, the Caucasus (perhaps the Lebanon and the Atlas), and the Elburz Mountains, to their not having been in existence during the Glacial age, of whose unmistakable handiwork they bear no adequate traces.

I have endeavoured in the recent papers which you have done me the favour to print to meet the demand of those geologists who have asked me for a cause competent to produce such a diluvial movement as I have postulated at the close of the Mammoth age, and which seems to be attested by a great mass of evidence. I have tried to collect a certain number of facts to show that at the close of the epoch in question there was a very violent and widespread dislocation of the earth's crust, which led to the upheaval of some of its loftiest mountain chains. This upheaval was accompanied, as I believe, by an equally rapid and substantial subsidence in other places, of which also there is much evidence, some of which you

may perhaps permit me some time to print. It was in my view by a combination of these two movements that the diluvial phenomena to which I refer were produced. This cause is at all events an efficient one, and is therefore not like so many of the physical causes appealed to by the current school of Uniformity, both inefficient and transcendental.

V.—On certain Gneisses with Round-grained Oligoclase and their Relation to Pegmatites.

By George Barrow, F.G.S.,
H. M. Geological Survey.

[Communicated by permission of the Director-General.]

In the course of my work in the Highlands of Forfar, I have been much struck with the mode of occurrence of certain light-coloured gneisses, of undoubted igneous origin.

They are intruded into the surrounding rocks in an infinite number of thin bands or sills, generally interlacing, and often not more than two feet thick; sometimes not exceeding an inch. Their bulk in some areas exceeds considerably that of the older rocks, at other times it is far less.

Two points are easily noted; first, they have no selvage edge; secondly, they have a very characteristic aspect, due to rounded grains of oligoclase.

Commencing in an area where these characters are well marked, we find that the rock consists of oligoclase, muscovite, biotite, quartz, and microcline, but the last mineral bulks far less than the more basic felspar. It is the oligoclase that is so round-grained in form, and gives the rock its characteristic appearance. The micas, especially the muscovite, may easily be seen to have sharp angles.

Going southwards, that is, away from an area where gneisses predominate, we observe the round-grained character to become less marked, and the oligoclase to form a smaller proportion of the whole rock. Still further south, the gneissose character becomes less marked, the oligoclase is further diminished in amount, while muscovite begins largely to exceed the biotite.

In this phase the rock begins to be much permeated with coarse pegmatite, which forms a massive fringe to the southern edge of the gneiss, this fringe attaining in one case a breadth of 700 yards. The pegmatite consists essentially of microcline, quartz, and muscovite, the oligoclase being usually very small in quantity, and not visible to the unaided eye. That this rock consolidated much as we now see it is obvious from the vast area of contact metamorphism that accompanies its intrusion. How then was it produced? I learned the solution of the enigma from a normal granite. A common type of granite consists of potash-felspar, quartz, oligoclase and two micas; the first-named mineral usually attaining the greatest size, and being most striking to the eye. But examine

a broad dyke, belonging to the same original magma, and its appearance is seen to be very different. The potash felspar is no longer visible to the unaided eye, but it forms with the quartz a finely granitic matrix, in which the earlier crystals of consolidation, the two micas and oligoclase, are porphyritically embedded. Still further, if the rock is slightly decomposed, the latter mineral, somewhat tabular in form with conspicuous development of the clinopinacoid, is seen to have an outer shell, fairly well marked off from the much more *rounded inner portion.* The oligoclase obviously came up in this form, and completed its growth from the magma in which it lay, which later portion has been shown by Prof. Sollas[1] to be more acid in character than the core. The round-grained crystals then, of our round-grained gneiss, want their outer rim of later growth. How is this to be accounted for? If such a granite-magma be intruded during or towards the close of a powerful earth-movement, it may be forced, by the tremendous pressure, into every possible plane of weakness in the surrounding rocks. Now, obviously, it is not the crystals, but the liquid portion of the rock that must enter first, and all the crevices must be opened to the diameter of the oligoclase crystals before any of them can enter. The acid portion then of the magma, containing the constituents of potash felspar, must travel somewhat in advance of the crystals of earlier formation. In addition, the continuance of the pressure will still further force the liquid from the solid crystals, leaving at last just sufficient of the magma to fill the interstices between them. Thus, in my opinion, has the perfect form of round-grained gneiss been produced, which is by no means uncommon. The more acid magma will travel furthest, and finally consolidate as pegmatite. Put back the pegmatite into the gneiss, and you have the composition of a normal granite. The round-grained character of the oligoclase is thus seen to be due to the draining off of the magma, from which it normally finishes its growth.

To turn now to the question of the absence of selvage edges to the gneiss. Obviously a coarse-grained intrusion of an inch in thickness would only be possible if the country rock were nearly at the same temperature as the magma. There is further evidence tending to this conclusion, but as some parts of it are matters of dispute, it may be omitted for the present.

From the foregoing remarks we may draw the conclusion that neither a round-grained gneiss of the above type, nor its accompanying pegmatite, is in any true sense a metamorphic rock; both consolidated as we now see them. In addition, it would be more accurate to describe the pegmatite as being *extruded from* the gneiss, than intruded into it. The latter view implies a later date, while both are obviously of the same age. Like any other rock, such gneisses and pegmatites are liable to *subsequent* metamorphism.

[1] See Prof. Sollas' paper, "Contributions to a Knowledge of the Granites of Leinster," Trans. Roy. Irish Acad. vol. xxix. p. 427.

VI.—On the Continuity of the Kellaways Beds over extended Areas near Bedford, and on the Extension of the Fuller's Earth Works at Woburn.

By A. C. G. Cameron;

of Her Majesty's Geological Survey.[1]

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During late years, by the opening of new sections, fresh information has been obtained of the geology of different parts of the country, and several fine excavations, the result of railway enterprise, have afforded great sections of the Kellaways beds in localities where only conceptions of them previously prevailed. Gaps have been filled up, and the continuity of the beds over extended areas confirmed. The principal revelations come from the Hull and Barnsley, and from the Swindon and Cirencester railways, opened respectively about 1881 and 1883, and the works now in progress in connexion with the widening of the main line at Oakley, near Bedford. There are records too of deep sinkings and borings, away from the outcrop, that indicate an area for the Kellaways as extensive as that of the Oxford Clay itself—and in the Midlands a more than usual thickness is reported; the Bletchley boring of 1886 specially indicating this.

From Wiltshire to the bold cliffs of Gristhorp and Scarborough Castle, along the great sweep of Oxford Clay, this immense sand-bank—if it is such—is well enough indicated, and, when hidden beneath the Fens, the Humber flat, or the Drift, is unquestionably accounted for.

Probably the earliest reference to the Kellaways comes from Boston, in Lincolnshire, where, in 1783,[2] the Oxford Clay was penetrated 470 feet, and then sand, to a depth of 8 feet, when the boring was abandoned as the water which then rose proved to be salt.

About that time, too, William Smith observed at Kellaways Bridge, in Wiltshire, a stone being quarried for road-metal that "occurred in irregular concretions, the exterior aspect of which is brown and sandy, the interior being harder and of a bluish colour. It consists almost entirely of a congeries of organic remains. The beds of clay which cover this rock abound in selenite, and below are beds of clay again."

The late Mr. Bristow informed me that there is no Kellaways Rock now at Kellaways, it having been all quarried away at its outcrop, for road-stone, years ago. Local stones were much more used in olden times than now, owing to the difficulty of transporting road-metal; thus the rock at Kellaways was then used for that purpose.

Briefly reviewing the past literature of this formation we find that, owing to the absence of sections along its course and the difficulty in consequence in tracing it, it was alluded to as being of

[1] An abstract of this paper was read in Section C, British Association, Cardiff, 1891.

[2] Phil. Trans. vol. xvi. p. 183 (1785).

partial occurrence only, and, if not entirely wanting in the Midland districts, at least so attenuated as to be inseparable from the Oxford Clay. For reasons such as these the Kellaways were not known in Bedfordshire until of late years—nothing of the kind having been recorded; the preponderance of yellow sand and loam on the sides of the valley around Bedford being looked upon as an extension merely up the slopes of the sand and gravel prevailing in the town. Large areas of building land have recently been covered with houses at Bedford, and there is a constant demand for bricks and lime. Brickmaking is carried on with energy, and the brickfields and stone-pits are prominent objects of interest in the neighbourhood of the town. Pits are opened at the outcrop of the Kellaways and carried down into Great Oolite through Lower Oxford (selenite clay), Cornbrash and purplish Cornbrash clay. In the making of bricks the Lower Oxford and the Kellaways loam or "lam earth," as it is called, are mixed, lessening the liability in the clay to contract and crack in drying, as happens when the Lower Oxford, the 'strong' clay, is used alone.

These Oolite divisions are seen to great advantage in the valley of the Ouse, as they lie upon each side of the river. Those extraordinary concretionary stones that characterize the Kellaways, jut out in the valley and stand about in some brickyard sections, in clusters, like gigantic fungi.

There is an admirable display of these singular stones at Oakley Hill, near Bedford. The railway passes in a cutting there through a tongue of land that juts out into the valley from the amphitheatre of hills around, and the cutting which is now being widened and cut back towards the hills, has laid bare a considerable length of Upper Oxford, Kellaways rock and sand. The base of the Kellaways is not reached, but outcrops lower down with other underlying beds, round the flanks of the hill. Big stones, smooth and rounded, stand out in relief in the sides of the cutting, the effect being heightened by the softened aspect and sombre hues of the clay above. These also stood in rows upon the sand before being broken up, to make way for the rails. One of the workmen likened them to boulders put down for stepping-stones, adding that there must have been "a flood there at one time." Some of these huge stones measure thirty feet in circumference. Where the sand is dug away and they stand each upon its own pedestal of sand, the resemblance to prodigous mushrooms is almost more than fanciful. Sometimes two stones are joined by a neck forming twin stones, when this semblance is lost in the figure of an hour-glass or the number eight. Where many stones are near together—and there are twenty or thirty now and then—these all but form beds of hard sandstone.

In some cases they are isolated and bare on the bright yellow sand, and the mind reverts to the sea and the sands and the blocks of stone one sees there when the tide is out.

Prof. Harker gives an account of the Kellaways beds exposed in the railway cutting at South Cerney, near Cirencester, and with these the beds at Oakley are identical. The shelly bands in the Bedford

sections have, however, a direct bearing on the concretionary origin of these stones, as set forth by Prof. Harker, that is not mentioned in his paper. The Upper Oxford and Kellaways beds in the Bedford sections are divided by a shelly calcareous band in contact with a shelly cap to the concretionary stones. Where this line is a broken one, there is no development of concreted rock below, and by gradations the Oxford Clay passes into silt and sand, seemingly indicating the piling up of the shells at particular spots as would happen on a shore exposed to strong currents; the subsequent decomposition of the animals resulting in the sand beneath being concreted into hard rock, as Prof. Harker says. The sand seems to have had deep water over it, as there do not appear to be any fossils whatever in it. On the other hand, the concretionary stones are—externally anyway, as Mr. Smith says, "congeries of organic remains."

All the animals, except the Belemnites, seem to have died young; *Serpulæ*, so conspicuous on other Oolite shells, are entirely wanting. There are shelly bands wholly made up of *Belemnites* and *Gryphæa*, while *Ammonites*, *Aviculæ*, and other forms besides, adhere to the upper surface of the doggers in great profusion. Pieces of this rock are more like Kentish rag than any other stone I know. A variety of *Gryphæa dilatata* occurs in such abundance and so like *Gryphæa incurva* in the Lias, that one would think the destruction of these beds could have contributed shells of *Gryphæa* to the Drift, as easily as the Lias. Although a hard rock when dug at any depth, the Kellaways becomes rapidly friable and crumbles on exposure; and where these stones jut out naturally, the shelly cap is usually gone.

There is an indurated seam of sandy marl above the shelly band at Oakley, which breaks into conical forms, exhibiting the structure known as cone-in-cone. The broad surfaces of these cones are upwards, ending against the Oxford Clay.

I saw no instance of the apices pointing towards each other; and the whole series stand vertical in the stratification with the points downwards, or to the bottom of the seam. It is difficult, therefore, to conceive of these cones being due to the upward escape of gases —as suggested by Mr. John Young of Glasgow,[1] although these would be emitted; the decay of the dead animals being sufficient to generate gases in the deposit whilst the bed was in process of formation.

Throughout this seam the texture of the stone is more or less fibrous, and in the phenomena of the cones the fibres seem united into tufts, which taper downwards and end in a point; rather resembling some stalactitic infiltrations.

The tract of land occupied by the outcrop of the different Kellaways beds, although of necessity only a narrow band, presents changes in the character of the soil marked enough to be known by distinct local names.

[1] Geol. Mag. Vol. II. June, 1885, on Cone-in-Cone Structure. Abstract of paper read to Geol. Soc. Glasgow.

The long narrow sandy piece on the slopes that reach the Boulder-clay (and its breadth expands in places) is 'white land,' easily recognized even at a distance in dry weather, from its colour. Again, the zone next the white land, and between it and the stony (Cornbrash) soil, farmers and others call "lam earth" or loam. This is the horizon, too, in which the brickfields are opened.

There is nothing in these soils, viewed agriculturally, to force the growth, and they are therefore almost useless for cropping; yet it is healthy grazing land, and cattle do well on it. Compared with other mild clays, the coldness of the "lam earth" or silt is in excess. It is not unusual to find osier beds attached to the brickfields, and willows planted at particular spots where any considerable development of silt occurs; in such cases osier beds thrive on the hill-sides equally well with those upon the water-meadows.

Mention has been made of a saliferous rock in Bedfordshire, in the lower part of the Oxford Clay, yielding a saline water containing large quantities of salt. It is doubtful whether the true seat of this water is in the Kellaways or in the underlying Oolites. It seems, however, certain that the saltness is considerable where the Kellaways is thicker than usual. At the same time, even a larger quantity of chlorine existing in the form of common salt occurred where the water was obtained from rocks that underlie the Kellaways beds.

The great value of our salt springs is for baths, and therefore the directors of an establishment in the educational centre from which I write, are to be congratulated on their recent discovery. There is no reason for ascribing any unusual thickness to the Kellaways beds of Bedfordshire; nor any serious quantity of salt. I think myself that the "sandy bed with big stones" is well known to well-sinkers in the clay districts as a trustworthy and palatable water-bearing stratum, beyond which they have no need to sink—that is for the requirements of any ordinary supply.

The borings at Swindon and Bletchley, as a search for water, were failures; the water in both cases being very salt. Far down, however, in these bore-holes great developments of Kellaways beds were discovered under several hundreds of feet of clay. Concerning Swindon, the publications in 1886 (the boring was made in 1875) show the water to have come up from the Forest Marble, and it was near this horizon that the water at Bletchley was found.

Notices of this boring appeared[1] in connection with a report that the borers, after passing through the Oxford Clay, had come upon granite or granitic rock.

Pieces of granitic rock did come, it appears, from these depths, but there is no proof that the whole of the thickness assigned to granite consisted of that rock. I passed all the samples myself as Kellaways sand and stone. It has, however, been suggested[2] that

[1] See Geol. Mag. Dec. III. Vol. IV. p. 139 (1887), Prof. Hull's letter; Dec. III. Vol VI. p. 356 (1889), A. Jukes-Browne, On the Occurrence of Granite in a Boring at Bletchley.

[2] Geol. Mag. Dec. III. Vol. VI. p. 360 (1889), Granite in Boring at Bletchley, A. Jukes-Browne.

the Kellaways beds beneath Bletchley may include granitic blocks and boulders embedded in the sand.

The hardness of the Kellaways when dug at any depth might well account for the incessant pounding and tediousness in making this boring. Prof. Harker describes the Kellaways stone as "obstinate," blunting the chisel of the excavators, and resisting any forces but dynamite and gunpowder. Should other borings afford no further information, Granite in the Bletchley Boring will remain unique in our local geology, and justly excite much wonder.

North of the Humber, where the lands bulge out at Drewton-on-the-Wolds and overlook the Humber flat, there is a greater thickness of Kellaways sand than has been observed throughout the central plain of England. At the Drewton cutting of the Hull and Barnsley railway, there is a tendency throughout of the sand to consolidate. Big boulder-like stones are not, as before, the most prominent feature, being replaced by blocks of tough siliceous sandstone weathering out in planes amongst the softer beds of sand. Above are fossiliferous beds of hard ferruginous sand, giving a very brown and irony look to this cutting.

From this point northwards the rocky character of these beds is maintained, and the Kellaways, if lacking in the peculiarities developed in the south, leave England with importance on the Yorkshire coast.

Since writing the above paper I see that Conybeare and Phillips, in describing the Lias, state that "irregular beds consist of fibrous limestone and cement stones (septaria), so called because used in making Parker's cement. Where the fibres are not parallel to each other, they often form that irregular substance so common in the Coal-measures, to which an organic structure has often erroneously been attributed and termed the cone-in-cone coral."[1]

Observations on the Extension of the Fuller's Earth Works, at Woburn Sands.

Fuller's-earth possesses remarkable grease-absorbing and cleansing properties, which places it foremost amongst the list of mineral detergents. The water thrown out by this formation is very soft and pure, and blocks of the earth have been placed in wells to purify the water. A superior quality of Fuller's-earth is procured from the Lower Greensand at Apsley Heath, in Bedfordshire.

Until lately the means adopted to work this mineral consisted simply of cylindrical holes in the sand, called "earth wells," or merely "wells," dug down to the Fuller's-earth, and without lining of any sort, and of these there were but two.

The operations now in progress bid fair, however, to give rise to no small stir in the place.

On the brow of this hill are the offices of the Fuller's-earth Company, with kilns for drying, and grinding mills used in the preparation of the earth. Sinking and shafting and the laying-out of underground works are carried on; and the little waggons with

[1] Outlines of the Geology of England and Wales, p. 264 (1822).

their loads of "earth" are gliding up and down the steep hill-side. Apart from its being a source of wealth and industry in the country, this extension at Woburn of the Fuller's-earth works is an interesting item in Bedfordshire geology, and the borings will determine probably the general arrangement of this mineral, regarding which, up to the present time, no particular notice has been taken.

VII.—The Manufacture of Serpentine in Nature's Laboratory. A Reply.

By Major-General C. A. MacMahon, F.G.S.

PROF. BLAKE, F.G.S., President of the Geologists' Association, has been good enough to send me cuttings from his recently published work "Annals of British Geology" containing his running comments on papers by me published during the period embraced by his volume, and for which I desire to tender him my best thanks.

I think it will be more courteous on my part to offer a few observations by way of reply to Prof. Blake's criticisms than to allow them to pass in silence; but I must confine myself to one paper, by way of sample. I select that bearing the title at the head of this communication (published in the Proc. Geol. Assoc. vol. xi. p. 427), as the subject discussed in it is of general interest to geologists.

The first extract from the Annals of British Geology I select for comment runs as follows:—

"He then enters into the chemical question, and explains how carbonic acid will decompose silicates of magnesia and iron. The principal mineral altered being olivine, he shows that if from 2 molecules of this $(MgFe)_4\ Si_2O_8$, one molecule of ferro-magnesian oxide is removed, MgFeO, and two molecules of water added, H_4O_2, we get Serpentine, $H_4\ (MgFe)_3\ Si_2O_9$, but 'as this involves an increase of volume some silica may also be removed.' This last suggestion will, of course, alter the above, and bring it into harmony with the method suggested by Roth, as quoted by Teall (Brit. Petrography, p. 106), *i.e.*, 5 molecules of olivine=10 MgO+5 SiO_2 −(4 Mg O+Si O_2)+4 H_2O=6 Mg O,+4 Si O_2+4 H_2O=2 molecules of Serpentine."

The sentence "as this involves an increase of volume some silica may also be removed" is marked by inverted commas and professes to be a quotation from my paper. Prof. Blake employs inverted commas, apparently, in order to convict me of a blunder out of my own mouth; the word "also" implying that some silica may have been removed from the *two molecules* of (MgFe) O_2· Si O_2 in addition to the molecule of ferro-magnesian oxide; and as the loss of this silica is not shown in my account of the *two molecules* of olivine, my calculation must be wrong.

Strange to say, the passage quoted under inverted commas does not occur in my paper. Not only did I not write the sentence given as a quotation, but it involves a serious misrepresentation of the views expressed in my paper.

What I actually said is as follows: "As the above change [viz. the conversion of two molecules of (Mg Fe) O_2. SiO_2 into H_4 (Mg Fe)$_3$ Si_2O_9] appears to involve an increase of volume, I think it probable that the passage of carbonated water through olivine results in the removal of some of the ferro-magnesian silicate [viz. *Olivine*] (without conversion into serpentine) in the form of soluble silica and carbonates." My argument then was this: Olivine is a silicate of magnesia and iron. Its conversion into serpentine involves hydration and an increase in volume; it is therefore probable that a portion of the olivine is removed *without conversion into serpentine* in the form of soluble silica and as carbonates of iron and magnesia. My account therefore (to borrow a book-keeping term) only applied—and I thought that was sufficiently evident—to that portion of the olivine that remained behind and suffered conversion into serpentine.

Suppose a parent gives a gold sovereign to his son but at the same time takes back from him five shillings in silver; what would be said of his logic if the parent subsequently called upon his son to render an account of his expenditure, and proceeded on the assumption that the boy had spent twenty shillings of his parent's money on "tuck"? Would not the boy be right in saying, "Why, I gave you back five shillings, and I have only to account for fifteen!" Yet the parent's position in my illustration is Professor Blake's position with reference to his criticism on my paper. I have humbly tendered my account of the fifteen shillings—straight and square—but Prof. Blake says, in effect, "That is all very fine, but your account of the fifteen shillings must be wrong, because you admit having given us back five shillings out of the sovereign!" Those are not his words, but that is what his criticism comes to.

In another part of his abstract Prof. Blake writes as follows:—

"The entrance of water into permeable rocks is easy to understand, but beyond this, it finds its way into the heart of the hardest minerals. The writer introduces Boscovitch's theorem, that molecules within a certain distance have a repellant rather than an attractive force, in order to show that there must be molecular interspaces in minerals. Since the alteration of these minerals begins sometimes in the centre, when the channels through which chemical constituents have been abstracted or introduced are too small to be revealed by the microscope, he thinks they must have come in by these invisible pores [in which case, why do they not alter the outside as they pass by?]."

The objection raised by Prof. Blake is one easily met. Now-a-days, when we can not only study thin slices of rocks under the microscope, but also isolate the minerals of which these rocks are composed, and subject them to chemical analysis apart from the mass of the rock, petrologists have come to recognize the fact that a gradual change, more or less pronounced, frequently takes place in the chemical constitution of the uncrystallized magma during the gradual cooling and consolidation of an igneous rock. Some minerals crystallize in advance of others, and in doing so withdraw

from the magma a lion's share of some of the chemical constituents. It may be a selfish thing to do, but, in this sense, some minerals are habitually selfish. The usual rule is that the more basic minerals separate first, and consequently those last formed are more acid than the minerals of the "first generation."

Owing to the progressive alteration of the magma brought about from the above cause, a gradual change in the composition of minerals of slow growth may, and often does, take place in the course of their formation; and this modification is marked by corresponding changes in their physical characters, such as colour in transmitted light, and optical properties. Hence what is known to microscopic-petrologists as zonal structure is commonly observed in the minerals of certain rocks—a structure so marked that it sometimes carries with it a progressive change in the angle of extinction from the centre to the periphery of the crystal, indicating in extreme cases a change in the mineral species. Now where, as explained above, the magma becomes more and more acid as comparatively basic crystals separate out from it, the centre of a mineral of slow growth would be more basic than its peripheral portions; and the centre would be more susceptible to the attacks of aqueous agents than the periphery, because the more basic a mineral is, the more readily it succumbs, as a rule, to ordinary corrosive agents.

Variations from the normal type are even more common in the mineral than they are in the animal world, and a very slight difference, no matter from what cause it may arise, in the composition of one crystal as compared with that of another of the same species, or in one part, as compared with another part, of the same crystal, may suffice to give temporary immunity from the attack of a highly dilute acid. The corrosive agent makes for the weak spot, and exhausts itself on the material there before it attacks the less susceptible material in other places. Hence we often see in the examination of thin slices of rock under the microscope, that whilst one olivine has been more or less completely converted into serpentine, another by its side has been left untouched; and whilst part of a crystal has been converted into the hydrated mineral, the rest has successfully resisted conversion into serpentine. Had the process not been arrested, the less tractable olivines would ultimately have been conquered.

In view of the above facts, if we grant that water can find its way through the pores of a mineral, I see no difficulty in understanding why the central portions of some minerals should yield to water charged with carbon dioxide, or other chemical reagents, before the peripheral portions.

I have not alleged, however, in my paper, that the central portion is always the first part attacked. I wrote:—"This alteration does not always begin at the outside of a mineral, and every microscopic petrologist will be familiar with cases in which it has been set up at the heart, and in the internal tissues—so to speak—of a mineral, and when the channels through which chemical constituents have

been abstracted, or have been introduced, are too small to be revealed by the microscope." As a matter of fact, the alteration sometimes begins from the outside. In cases in which an olivine is perfectly homogeneous in chemical composition, one would expect this to be the case—the attack begins from the outside, and works its way slowly and gradually into the interior through "planes of easy solution," or any other planes of weakness, that may exist. When there are actual cracks, the liquid reagent is not too proud to avail itself of the aid afforded by them in the work of sapping its way into the heart of the fortress.

As for the question whether water under pressure, and under the conditions that obtain below the surface of the earth, can penetrate into the inner pores of a mineral, I must refer the reader to my paper under discussion, and to the instructive papers by Prof. J. W. Judd, F.R.S., on schillerization and kindred subjects. "We must never forget," he writes in one of his papers,[1] "that in the deep-seated rocks . . . . the whole mass, crystals and base alike, must be permeated by liquids and gases." Cracks we can see under the microscope readily enough, and no petrological microscopist is likely to ignore their existence or the part they play in the circulation of underground waters. I likened them in my paper (p. 431) to the small veins and arteries in the human body; but we want something in the mineral world to correspond with the minute capillary pores through which blood finds its way between the ultimate cells of which the animal body is built up. We cannot see the atoms, or even the molecules, of which minerals are composed; but we can infer the existence of molecular interspaces, and we can assure ourselves of the fact that water, under pressure, actually finds its way into these molecular interspaces by what lawyers would call circumstantial or indirect evidence. For instance, when we find *hydrous* minerals occurring in the interior of the mineral constituents of igneous rocks, and when we have independent evidence that these hydrous minerals are of secondary origin, we must admit that water worked its way into the heart of the altered mineral after its birth, unless we are prepared to show that the parent crystal originally contained enough water to supply the total quantity contained in the parasitical minerals generated in its tissues, as well as *all* the chemical constituents contained in them. The subject is too large to enter into here.

Prof. Blake proceeds as follows:—"Minute cracks, however, are also present, as is shown by granite and greenstone absorbing water, and containing air, and the capillary action is increased by pressure and heat. [He seems to think, however, that the same conditions would facilitate the introduction of water into the intra-molecular spaces, since] 'heat signifies an increase in the force of repulsion that keeps apart the atoms [*sic*] and molecules of which these minerals are composed' [in which case, heating should make a compound take up more water, but, *e.g.*, 'the hydrate of sodium sulphate is more and more thoroughly converted into the anhydrous salt as the temperature increases.' (Fownes.)]"

[1] Q.J.G.S. 1889, p. 181.

From Professor Blake's insertion of "[*sic*]" after atoms, I infer that he regards a molecule as a homogeneous, solid body like a bullet, as it appears to our unaided vision; if so, his conception of the constitution of a molecule can hardly be considered "up to date," as the following brief extracts from two well-known works will show. "Atoms cannot be divided physically; they are retained side by side, without touching each other, being separated by distances which are great in comparison with their supposed dimensions. A group of two or more atoms forms a molecule."[1] "Nor should it be forgotten that, granting the fundamental hypothesis of the molecular and atomic theory, and also granting that each atom can directly interact with a limited number of atoms in a molecule, we are obliged to regard the atoms which form any molecule as performing constant regulated movements, and not as might be supposed by a careless or superficial reader of the atomic explanation of isomerism, as in absolutely fixed positions within the molecule."[2]

According to modern conceptions, therefore, the atoms which constitute a molecule are linked together, but the linking is analogous to that of moons to a planet, and of a planet and its moons to the sun.

The illustration which Prof. Blake adduces, at the end of the above extract, in support of his objection, seems to indicate a fundamental misapprehension on his part. The case of the hydrate of sodium sulphate seems to me to have no bearing on the point at issue. In my paper I discuss the mode in which the *hydration* of a specified silicate is brought about in a rock below the surface of the earth in the presence of water under considerable pressure; and Prof. Blake opposes the case of the dehydration of a salt by heat under ordinary atmospheric pressure at the surface of the earth, and in the absence of water. There seems to me to be no analogy between the two cases. We all know that at the surface of the earth calcium carbonate can be converted into lime (CaO) by raising its temperature to a certain point; because at that critical temperature, the force of repulsion, generated by the heat, overcomes the force of the attraction between the calcium oxide and the carbon dioxide, and the latter passes into the gaseous state. Under plutonic conditions, on the other hand, the carbon dioxide remains in union with calcium oxide, and crystalline calcite is formed.

In my paper I was considering the question of capillary flow under heat and *pressure*. I showed that, "although the pressure under which water is put in circulation through the capillary pores depends on the head" ("·43 lbs. per square inch per foot in height"), "the freedom with which this water flows through the capillaries" must, with reference to the experiments of Poiseulle, be increased by heat. I contended, therefore, that if "the pressure under which water is being injected into the pores of a mineral remained constant, heat would facilitate the capillary flow through those pores

[1] Ganot's Elements of Physics, by Atkinson, p. 1.
[2] Principles of Chemistry, by Patteson Muir, p. 154, footnote.

by reducing the resistance to the passage of the water." It is no answer to this contention to say that under totally different conditions water is driven off from the hydrate of sodium sulphate by the application of heat. Prof. Blake might as well argue, it seems to me, that marble could not have been formed under plutonic conditions, because, at the surface of the earth, and under the pressure of one atmosphere, the application of sufficient heat will result in the carbon dioxide being driven away from calcium carbonate. It does not require much chemical knowledge to know that if you alter the conditions you may expect to obtain different results.

VIII.—A Reply to Prof. Blake's Comments on Inferior Oolite Ammonites.[1]

By S. S. Buckman, F.G.S.

PROF. BLAKE'S book is a valuable work of reference; but the criticisms appear to be both hurried and inaccurate. In the notice of my Monograph the title is incorrectly given: there are certain other clerical errors; and some mistakes which a little more investigation would have prevented.

While placing his comments in brackets, it is unfortunate that Prof. Blake has not found some means to distinguish between remarks based upon the author's words, and statements of his own. This is especially noticeable in the "critical digest" of *Haugia*, where remarks, which appear as if they originated with Prof. Blake, are really my statements in another form.

Some of Prof. Blake's principal comments invite reply. For the sake of brevity I will place them in italics between inverted commas.

"*The meaning of species and genera . . . . is of the most restricted kind . . . their distinctions arbitrary.*" As to the species, the charge does not seem to be sustained in the part reviewed; for out of twenty-seven species described I am only answerable for five. I have also combined as one species forms which a German author regarded as three; and in other cases I appear not to have made enough species to please my critic.

The genera are restricted I own; it is part of the plan of the work. I regard species as various developmental gradations. I look upon genera as groups of species in more or less direct genetic connexion, possessing certain features in common. Since species in direct genetic connexion—and therefore genera—arose one from another by the accumulation of successive slight modifications transmitted in accordance with the law of earlier inheritance, the distinctions between species or genera in direct genetic connexion must be arbitrary at certain points. I have always admitted this; but between the homoplastic developments which result from the operation of similar economic laws on the heterogeneous descendants of a remote common ancestor, the distinctions are not really arbitrary,

[1] The Annals of British Geology, by J. F. Blake, M.A., F.G.S., p. 308, 1891.

though they may appear to be so. It is unfair to adversely criticize genera by comparing, say, the senile metamorphoses of different phylogenetic series. Such degradational forms have lost, to a certain extent, the special features which distinguished the acmic species of their genera. But a system which recognizes the true biological relations of these homoplastic forms by placing them in separate genera is far less arbitrary and far less unnatural than the Waagenian system which Prof. Blake introduced to English readers,[1] in which homoplastic forms were arbitrarily dragged into the same genus on account of similarity of outward shape, while their true genealogical affinities were completely misunderstood. My efforts towards a natural system of grouping founded on an interpretation of Ammonite genealogy may not be correct at their first start; but I hope we may never return to so unnatural a grouping as was expressed by *Aegoceras*, *Arietites*, and particularly *Amaltheus*. "*H. occidentalis has no tubercles, [and therefore should belong to another group]*." Why? No new feature is introduced. There are no tubercles in senile "*variabilis*," or in adult "*jugosa*," and none in "*occidentalis*" at any stage. It is an illustration of the law of earlier inheritance. Had this loss of a feature been accompanied by the appearance of some new character, I should have been inclined to erect a new genus; but in a simple case of decadence like this I did not see the necessity.

A definition by Dr. Haug is given, with which, it is said, my figures do not agree, as they show no fasciculed ribs. I do not know if this be a case of my descriptions "*not agreeing with the author's original definition*"; but this is not original—it is a quotation from a letter. I have pointed out (page 154) that my figures do not bear out these remarks; but in Haug's *original* definition and figure of *H. occidentalis* fasciculed ribs are not noted or shown.

"*H. Eseri. [The author's figures include several species, none of which agree with the type]*." What, not figs. 3, and 4, pl. 25 with Quenstedt, Ceph. pl vii. fig. 9 *a-b*?[2] Dr. Haug wrote to me, May, 1890, "Vos *H. Eseri* de la pl. 25 sont tout-à-fait typiques."

The suture-line "*fig.* 6, *pl.* 35," is not normal, the siphonal lobe being to one side.

"*G. mactra* and *D. Moorei*" are degraded forms, and not fair samples by which to test the genera.

"*G. aalense, vars. a–δ [no proof given that they are the same species]*." I have yet to learn how it can be *proved* that certain forms are of one species or not.

"*G. subquadratum identical*" with *Saemanni*, "*differences assigned not borne out by figures.*" Similar remarks are made about other species in this "critical digest." I will take this as a sample. The differences given are (in *subquadratum*) coarser, more reflexed ribs, stronger inner margin, deeper umbilicus more slowly-coiled" (page 202). I think that these differences are all very noticeable in the

[1] Ceph.; Yorkshire Lias, 1876.

[2] Allowance must be made for a slight discrepancy in the drawing of the inner margin in Quenstedt's two figures.

plate—especially the slower-coiling, if any one will measure the figures with compasses.

"*G. Saemanni* [*but apparently including as var. β another solid-keeled species*]." This is a most curious criticism in face of the explanation of the plate, and the fact that these forms are expressly included in the hollow-carinate section of the *Grammacerata*. I wrote to Prof. Blake asking him if he had mistaken the carina on the core (pl. 34, fig. 2) for the true carina. His reply rather astonished me—it showed that he did not understand the structure of a hollow-carina. He "thought it was the presence of a keel in the shell, but not on the cast," which made a hollow carina. This is certainly not the case: it is the presence of a partition-band overlying the abdominal part of the cast containing the siphuncle. The partition-band connects the inner walls of the overlying elevated carina, and so there is formed a hollow triangular tube around the periphery of the Ammonite. The abdominal part of the core may be carinate or rounded—this does not affect the structure of the hollow-carina in the least. I fully explained the structure of the hollow-carina in my Monograph, page 81, footnote, and illustrated its various phases in pl. A. In fig. 47 of that plate I figured a carinate core bearing a hollow-carina, and this feature is to be very frequently noted in the genera *Witchellia*, and *Sonninia*, as well as in *Grammoceras*.

A desire not to occupy too much space alone prevents me from replying to all Prof. Blake's criticisms; but from the above remarks I leave it to be judged whether the critic in an attempt to correct the author has not himself fallen into errors.

REVIEWS.

I.—Contributions to the Palæontology of Sicily.

The Crustacea of the Fusulina-limestone of the Valley of the Sosio River in the Province of Palermo, Sicily. By Prof. G. G. Gemmellaro.

I Crostacei Dei Calcari con Fusulina, della Valle del Fiume Sosio, nella Provincia di Palermo in Sicilia. Memoria di Gaetano Giorgio Gemmellaro, Professore di Geologia della R. Università di Palermo. Memorie delle Società Italiana delle Scienze fisiche e matematiche, Tomo viii. ser. 3, No. 1, pp. 40, and 5 plates. 4to. Naples, 1890.

THE author refers this Fusulina-limestone to the "Permo-Carboniferous" formation, and finds in it the following fossils.—I. Trilobites: *Proetus*, Steininger, 2 new species; *Phillipsia*, Portlock, 4 new species, and of the "subgenera" *Griffithides*, Portlock, and *Pseudophillipsia*, nov., one new species each.

If Prof. Gemmellaro is correct in his determination of these beds, then these Trilobites are younger than any hitherto discovered, and later than the true Carboniferous in age.

II. Macrurous Crustacean. *Palæopemphix*, gen. nov., with three new species. These carapaces doubtless belong to some small Macrurous Decapod, but it is not easy to recognize their ancestral relationship to *Pemphix* of the German Muschelkalk. The rostral portion of the carapace is curiously stunted (as in the *Crangonidæ*), and the antennal (antero-lateral) angles very prominent. These Sicilian forms are all seen *in profile*, but most of the early Carboniferous genera had the carapace flattened out, dorsally, as in the *Eryonidæ* of the Liassic and Oolitic period, and in the recent *Polycheles*.

III. Brachyurous Crustaceans. *Paraprosopon*, gen. nov., with one new species. This seems to belong to the genus *Cyclus* of De Koninck, and closely resembles *C. Jonesianus*, H. Woodward, in particular (see Geol. Mag. 1870, Vol. VII. Pl. XXIII. p. 558, Woodcut, Figs. 1–2).

IV. *Oonocarcinus*, gen. nov., with 3 new species. This form, according to the author, has its nearest ally in *Hemitrochiscus paradoxus*, Schauroth. It has, however, very much of the form of *Caryon*, Barrande, and reminds one of the head in *Sphærexochus*. But most of all *Oonocarcinus* resembles the coalesced segments of the buckler-like abdomen of the female in the *Leucosiadæ* (cf. Bell, Mon. *Leucosiadæ*, Trans. Linn. Soc. 1855, vol. xxi. p. 277, pls. xxx.–xxxii.).

Perhaps it may represent a part of an early Brachyurous Decapod; if so, it has an additional interest for us.

V. Ostracoda. *Cypridinella*, Jones & Kirkby, 2 new species; *Cypridellina*, J. & K., one new species; *Cypridella*, De Koninck, 2 new species; *Cypridina*, Milne Edwards, 2 new species; *Philomedes*, Lilljeborg, 1 new species; *Entomoconchus*, M'Coy, 1 new species; *Entomis*, Jones, 2 new species; *Beyrichia*, M'Coy, 1 new species. These have an exceedingly close resemblance to the true Carboniferous species of Britain and Belgium, and at first sight might in most instances be taken for them. The figured "*Beyrichia*," however, is a very doubtful specimen.

In bringing together the members of this most interesting local fauna, and illustrating it in so clear and admirable a manner, the author has done good service to Palæontology, and deserves our best thanks.

H. W. & T. R. J.

II.—British Pliocene Vertebrata.

"The Vertebrata of the Pliocene Deposits of Britain." By E. T. Newton, F.G.S., F.Z.S. Mem. Geol. Surv. United Kingdom, 1891, pp. i–xii, 1–137, Pl. I.–X.

THROUGH the generosity of the author we have been favoured with a copy of this valuable Memoir, which forms a companion volume to Mr. Newton's well-known work on "The Vertebrata of the Forest Bed Series of Norfolk and Suffolk," published by the Geological Survey in 1882. The Memoir also seems to have been prepared in connexion with another Survey publication by Mr. Clement Reid, "The Pliocene Deposits of Britain," said to have

been issued in 1890, but of which we have not yet been privileged to receive a copy for review.

The Forest Bed Series of Norfolk and Suffolk being included by the Geological Survey among Pliocene Deposits, the new volume contains many observations supplementary to Mr. Newton's former work on the Forest Bed Vertebrata; and one double plate is devoted to the antlers of Cervidæ, of which no illustrations were previously given. The principal interest of the Memoir, however, centres upon the vertebrate fossils of the English Crags, which are carefully subdivided into horizons in accordance with Mr. Reid's classification. "By far the larger part of the Vertebrate remains which are said to be from the Red Crag really come from the Nodule-bed (Bone-bed of some authors) which occurs at its base; and, further, a Nodule-bed with similar fossils is known to occur also under the Coralline Crag. Many of the fossils from the Nodule-bed have been undoubtedly derived from the denudation of Eocene strata, while others seem to be the *remanié* of Pliocene beds older than the Coralline Crag, but of which no traces are known to occur in Britain. It has been suggested that most of the Nodule-bed Vertebrates have been derived from Miocene strata, but there seems little evidence to support such an idea. Many Vertebrate remains have been found actually in the Coralline Crag and Red Crag above the Nodule-bed. The same is the case with the Norwich Crag, many specimens being obtained above the Basement Bed, or Mammaliferous Stone-bed."

Many new specimens are noticed and well illustrated in the course of the work, but the majority of Mr. Newton's observations have already appeared elsewhere, and they are now presented merely in a collected form, with references to the original places of publication. These references, indeed, with those also to other authors, form the only blemish in the work demanding serious adverse criticism. They are all placed within round brackets in the text itself, and render many pages quite unreadable, except by the closest study. The nominative is frequently separated from the rest of a phrase by one and a half or two lines. Perhaps, however, the saving of the extra cost of printing footnotes results in some advantage, for the Memoir is issued to the public at the unusually reasonable price of four shillings.

It would be impossible in the course of a brief review to do justice to the most interesting and valuable mass of information concerning the Pliocene Vertebrata now brought together. It must thus suffice to remark upon a few striking points. Mr. Newton considers that Mr. Lydekker is most probably correct in referring the teeth of *Hyæna* from the Red Crag Nodule-bed to the existing *H. striata*; and a right lower canine from Felixstow is now provisionally added to the evidence of the species. Owen's determination of the common Wolf from the Red Crag Nodule-bed is also confirmed, while an upper premolar and two canines are additional specimens recorded. *Canis primigenius*, Mr. Newton thinks, may be founded on a Cetacean tooth. The occurrence of *Cervulus dicranoceros*, first determined by Sir Richard Owen and since questioned by Dawkins and Lydekker,

is now admitted from new evidence; and the discovery of *Cervus elaphus* and *Cervus etueriarum* in the Forest Bed is definitely confirmed. Mr. Newton well remarks that "of two uncertain names it seems best to keep the one which has been so long in use, rather than introduce another equally uncertain;" and he thus describes the larger teeth of pigs from the Red Crag as *Sus antiquus*? Kaup. *Hipparion* is known only from the Red Crag Nodule-bed; and while agreeing with Mr. Lydekker that most of the teeth of *Rhinoceros* from this horizon may best be assigned to *R. incisivus*, the author considers that some may still belong to *R. Schleiermacheri*. *Elephas meridionalis* is definitely known from the Red Crag Nodule-bed, but *Elephas antiquus* has not been obtained below the Norwich Crag. There is still no evidence of *Mastodon* in the Forest Bed Series. To be "fashionable" Mr. Newton adopts the name *Microtus*, which literary "research" has lately shown to be applicable to the Voles; but "*Arvicola*" is placed in brackets to explain what is meant. The Cetacean remains from the Crag are treated in accordance with Mr. Lydekker's determinations; and the so-called tooth of *Ursus arvernensis* described by Lankester is ascribed to *Squalodon*. A vertebra and tooth of *Orca gladiator* from the Forest Bed are new, and another vertebra from the same horizon is referred to *Pseudorca*. The fish-remains are very fragmentary, but numerous, and the description of several distinct otoliths of *Gadus* is interesting. One tooth from the Coralline Crag is ascribed to the Wolf-fish (*Anarrhichas lupus*); and the occurrence of *Thynnus scaldiensis*, also in the Coralline Crag, is confirmed.

A tabulated list of species and a general summary conclude the Memoir. Notwithstanding the fragmentary character of all the remains, Mr. Newton's most painstaking researches have invested this summary with great interest and value. "It seems from a consideration of the Pliocene Vertebrata that the climate of England in the earlier part of that period was decidedly warmer than it is at the present day, and approached sub-tropical conditions; and that, notwithstanding minor variations which may have subsequently taken place, the general tendency was to become colder, so that in the Forest-bed times the climate was temperate, with, possibly, periods of greater heat and still greater cold, perhaps partly due to continental conditions, which at length culminated in the Glacial or Pleistocene Epoch. The earliest Pleistocene deposit recognized being the 'Arctic Freshwater Bed' of Norfolk, which is characterized by an assemblage of Arctic plants, and a *Spermophilus*, and occurs immediately below the Boulder Clay."

THE GEOLOGICAL SOCIETY.—The Medals and Funds to be given at the Anniversary Meeting of the Geological Society on February 19 have been awarded as follows:—The Wollaston Medal to Baron Ferdinand von Richthofen; the Murchison Medal to Professor A. H. Green, F.R.S.; and the Lyell Medal to Mr. George H. Moreton. The balance of the proceeds of the Wollaston Fund to Mr. O. A. Derby; that of the Murchison Fund to Mr. Beeby Thompson; that of the Lyell Fund to Mr. E. A. Walford and Mr. J. W. Gregory, and a portion of the Barlow-Jameson Fund to Prof. C. Mayer-Eymar.

III.—On the Formation of the Principal Groups of Iron-ore Deposits Occurring in Norway and Sweden. By J. H. L. Vogt. Transactions of the Geological Society in Stockholm, No. 138, Vol. 13, Part 5, May, 1891, pp. 476–536, Pl. 8.

Om Dannelsen af de vigtigste in norge og Sverige repræsenterede grupper af jernmalmforekomster af J. H. L. Vogt. Geologiska Föreningens i Stockholm Förhandlingar, Band XIII. Maj, 1891, s. 476.

THE author of this valuable communication has, in his work on slags, paid special attention to the conditions under which the silicates and other minerals are developed in magmas of varying composition. He is now extending the principles established by this kind of work to the igneous rocks of volcanic and plutonic origin. In the paper under review he deals especially with the origin of iron-ores of the Ekersund-Taberg type. These he attributes to "magmatic concentration" in strongly basic eruptive rocks; and in discussing the question of concentration he necessarily deals with matters of general interest to all students of igneous phenomena. Many of the facts referred to by the author have been described by previous observers, but they have never been grouped together so as to illustrate a general theory. Under these circumstances a somewhat extended notice of his paper appears to be thoroughly justifiable.

The author commences by remarking that ore-deposits may be divided into very well marked groups by taking into consideration the genetic principle which has been shown to be of so much value for classificatory purposes in other departments of natural science; and, after a general review of the principal Norwegian occurrences suggests the following classification:—

I. Ores formed by magmatic concentration in strongly basic igneous rocks.
II. Ores formed by pneumatolytic processes [*i.e.* a kind of subterranean fumarole-action; *e.g.* tin-stone deposits of Cornwall].[1]
III. Ores formed by sedimentation.
IV. Ores formed by metasomatic processes [*e.g.* Cleveland ironstone].
V. Ores formed by deposition of ferrous carbonate and other minerals in cracks.

In the consolidation of igneous magmas the ores and accessory minerals—magnetite, ilmenite, hematite, pyrite, magnetic pyrites, apatite, zircon, spinelle, titanite, perowskite, etc.—are first formed. Then follow the ferro-magnesian silicates—olivine, mica, pyroxene, amphibole—and lastly the minerals of the felspar group and free quartz. There may be a certain amount of overlapping in the minerals of the last two groups; but in a general way the order is that indicated above. Now the ore-deposits with which the author is immediately concerned can be accounted for by supposing that the chemical compounds of which they are composed became concentrated by diffusion-processes which are imperfectly understood at present. According to this view they would be analogous to the "basic patches" common in many eruptive rocks.

[1] The remarks in square brackets are interpolated by the reviewer. In what follows the author deals exclusively with the ores belonging to the first group.

As illustrating, in a general way, the effects of such diffusion, a dyke, 10 metres wide, occurring near Huk, in the Christiania district, is described. The centre of the dyke is a mica-syenite-porphyry, consisting of large crystals of orthoclase lying in a light red, fine-grained ground-mass. About one or two metres from the junction the ground-mass takes on a grey colour due to increase in the magnetite. As the junction is approached, the felspars decrease in size, plagioclase takes the place of orthoclase, and the ground-mass becomes darker and more compact. The actual margin is formed of a black rock, rich in iron, and containing a few porphyritic crystals of felspar (plagioclase) and mica. Pyrite increases as the junction is approached. The microscope shows that magnetite and biotite are very abundant in the marginal rock, which may be described as a kersantite. Chemical analysis establishes the fact that phosphoric acid is also more abundant at the margin than in the centre. The amount found corresponds to ·51 per cent. of apatite in the centre and 1·44 per cent. at the margin. There is nothing in the appearance of the dyke to suggest that there have been two intrusions. The transition from one type of rock to the other is gradual, and can only be accounted for by supposing that the magma became differentiated, before consolidation, by a concentration at the margin of the chemical compounds of the first formed constituents.

This case is so interesting that we quote below (1) the composition of the original magma as reckoned from actual analyses; (2) the composition of the centre; and (3) the composition of the margin:

SiO_2	62	63	47
Al_2O_3	17·5	17	20
Fe_2O_3 } ... FeO }	3	2·5	{ 7·5 { 5
MgO	3	2·75	5·5
CaO	3·5	3	7·5
Na_2O	5	5·5	4
K_2O	4	4·5	1
FeS_2	1	1	3

Having illustrated in this way the general principles of magmatic concentration, the author describes in detail the ilmenite-deposits of the Ekersund-Soggendal district. They are found in a region composed of plutonic rocks which may be grouped under the following terms:—labradorite-rock (a norite extremely poor in ores and ferro-magnesian constituents), hypersthene-norite, biotite-norite and enstatite-granite. Dykes (exclusive of the ore-deposits) traverse the district. They may be classified, in the order of their formation, as follows:—

I. Ilmenite-hypersthene-labradorite dykes of pegmatitic character.
II. Norite dykes.
III. Diabase dykes, generally containing olivine.

Titanite is absent from the labradorite-rock and the true norites, but is found in the enstatite-granite. All the above rocks evidently belong to the same general period. They are the products of the consolidation of one magma-basin.

The ilmenite-deposit of Storgangen may be regarded as forming a dyke 3 kilometres long, and 20 to 70 metres wide. It consists of

ilmenite, hypersthene and labradorite; and is, in fact, an ilmenite-norite. The average of ilmenite in the whole dyke is estimated at 40 per cent. In places as much as 70 or 80 per cent. may be found. The dyke is separated from the labradorite-rock by sharp boundaries. The same type of rock is found in several other portions of the norite-district; sometimes with lower and sometimes with higher percentages of ilmenite. As a rule it occurs under the same conditions as at Storgangen, and rarely passes over by gradual stages into labradorite-rock. In the true ore-deposits (Bläfjeld) the ilmenite predominates to such an extent that the rock loses its norite character altogether, and consists of 90 per cent. or even 95 per cent. of iron-ore. It occurs as dykes from two to six metres thick, and from one to two hundred metres long. Sharp fragments of the labradorite-rock occur as inclusions in the mass of the ore, thus proving conclusively that the veins are of later date than the surrounding rocks. Transitions may be observed between the ore-deposits and the pegmatitic dykes above referred to. The author gives several analyses of the ore, one of which we quote.

ORE FROM KYLAND.

SiO_2	44·05
Fe_2O_3	18·71
FeO	34·17
MgO	3·04
Total	99·97

A certain amount of magnesia is always found. This accords with the view that the formation of a magma, extremely rich in magnesia, is an intermediate stage in the formation of iron-ores by the process of concentration. The dykes of olivine-diabase and norite cut the ore-deposits.

Apatite is lowest in the labradorite-rock, somewhat more abundant in the ilmenite-norite (·05 per cent.), and reaches its maximum (5 per cent.) in the dykes of norite and olivine-diabase.

The ore-deposits of Taberg, in Småland (Sweden), described by Sjögren and Törnebohm, consist of titano-magnetite with olivine, traces of biotite, and a strongly basic felspar. The last-mentioned constituents are entirely absent from the richest ores. The mode of occurrence is different from that of Ekersund. The ore in this case is found *in the centre of a mass of olivine-hyperite into which it passes by insensible gradations.* The area occupied by the olivine-hyperite and the magnetite-olivinite (ore-deposit) is elliptical in form, and measures about one mile, by rather less than one-third of a mile. Similar rocks occur in other localities in Sweden and Dr. Wadsworth has shown that the ore of Rhode Island is of the same character.

That ore-deposits of the above type are the result of concentration in plutonic magmas is indicated by the following facts:—

(1) The different ores stand petrographically related to the surrounding rocks (*e.g.* magnetite-olivinite to olivine-hyperite and ilmenite-norite to labradorite rock).

(2) The ore in many cases passes gradually into the surrounding rock.

(3) In no case is there any evidence of the introduction of material by solutions or pneumatolytic processes. The segregations are characterized by the minerals of the surrounding rocks, and by these alone.

The author next proceeds to consider the main facts relating to concentration without reference to the causes which may have produced it, or to the chemical and physical conditions under which it has taken place. The iron oxides (ilmenite and titano-magnetite), which are the first minerals to form, are those which are most strongly concentrated. The ferro-magnesian compounds have also been concentrated, but not to the same extent.

The original magma of the labradorite rock of the Ekersund district may be supposed to have consisted of 2 parts ilmenite, 4 parts hypersthene and 94 parts labradorite; at a later stage of 6 Il. + 8 Hyp. + 86 La.; later still of 18 Il. + 16 Hyp. + 66 La.; again later of 40 Il. + 35 Hyp. + 25 La.; and lastly of from 80 to 95 or even 99 % of ilmenite, the remainder being hypersthene and labradorite. Similar phenomena are illustrated at Taberg and in the dyke at Huk.

Titanic acid is concentrated along with the iron oxides and enters into composition with them in the case of the segregations in basic eruptives. It is interesting to notice that in the Ekersund deposits the ore is ilmenite ($RTiO_3 nFe_2O_3$ whence R = Mg, Fe, Mn), which may be regarded as formed in part of a metatitanate; in the more strongly basic rocks of Taberg it is titano-magnetite ($Fe_2 TiO_4$ nFe_3O_4), which contains an ortho-titanate. In the former case the associated ferro-magnesian silicate is hypersthene, a metasilicate, and in the latter case it is olivine, an orthosilicate. In more acid magmas the titanic acid determines the formation of sphene, and the iron-ores if present are comparatively free from this constituent. In facts of this kind we see the influence of mass in modifying the chemical affinities.

Chrome-oxide, if present, is concentrated along with the basic minerals. The ilmenite of Ekersund contains chromium, and a little chrome-spinelle is found in the ilmenite-norite. Manganese is concentrated, but not to the same extent as iron.

Phosphoric acid may be concentrated; but not according to the same laws as iron, for it is more abundant in the dykes of diabase and norite than in the ore-deposits.

The position in the eruptive mass in which the rocks formed by concentration-processes are to be found is not the same in all cases. In the case of the dyke at Huk the basic parts form the margins; in the Taberg district the ore occupies a central position in the area formed of eruptive rocks; in the Ekersund occurrences it forms dyke-like masses which are sharply separated from the country rock.

The concluding part of the paper is occupied by a discussion of the processes by which concentration may have been brought about. Three ways are possible:—1. The minerals may be formed and mechanically collected in certain parts of the mother liquor. 2. The minerals may be formed and again dissolved in some other locality, thus producing a basic magma. 3. Concentration may take place by molecular diffusion without the actual separation of minerals.

The author considers that the first two methods have not been concerned in the production of the concentration which has given

rise to the Ekersund deposits, for microscopic examination shows that the minerals have been developed *in situ*—not mechanically collected—and there are no corroded grains such as might be expected if the second cause had operated. We are therefore limited to the third method.

The homogeneity of a solution may be destroyed by temperature-differences and by gravity. The influence of the former has been experimentally established by Soret, and follows as a necessary consequence of Van 't Hoff's theorem that osmotic pressure in the case of dilute solutions obeys the laws of gaseous pressure. The influence of the latter has been deduced experimentally by Gouy and Chapéron from the laws of thermo-dynamics. Where solutions become heavier by concentration, the lower part will be more concentrated than the upper part. The difference is slight, and can only be recognized with difficulty when a tube 100 metres high is used. The specific gravity of a molten magma will increase with an increase in the number of molecules of magnetite, ilmenite, magnesia-iron-silicates, pyrite, etc. Hence these molecules will, according to the law of Gouy and Chapéron, be more abundant in the lower than in the upper portion of a magma-basin.

Differences of temperature operating according to what the reviewer has elsewhere termed Soret's principle will cause the same molecules to accumulate in the colder portions of the same magma-basin.

Another cause which is believed by the author to be effective in aiding concentration is magnetic attraction. The different iron-compounds when dissolved in silicate magmas are, doubtless, para-magnetic. Under the influence of the earth's magnetism the molecules may become orientated; but, so long as they are uniformly distributed throughout the mass, there can be no concentration due to magnetic attraction. If, however, owing to temperature-differences, or to gravity, a local accumulation of magnetic molecules takes place, the magnetic attraction may cause the concentration to become more and more pronounced.

The dyke at Huk furnishes an excellent illustration of concentration due to differences of temperature. The magma of the ore-deposits of Ekersund and Taberg must, however, have been formed by concentration in the deeper portions of a magma-basin. A certain influence may be ascribed to gravity acting on the specifically heavier molecules; but it seems hardly probable that the important results which have been observed can be due to this action alone. Magnetic attraction operating constantly and for long periods may, however, when taken in connection with gravity, have given rise to the necessary amount of concentration.

The high specific gravity of the earth may be due to the concentration of the heavier molecules in the central parts, by the causes above referred to, during the earlier stages of the history of the planet. If so, the ore-deposits under consideration are the genetic equivalents of the originally molten kernel of the globe.

The intimate relation between the ore-deposits and meteorites has been especially commented upon by Wadsworth. J. J. H. T.

REPORTS AND PROCEEDINGS.

Geological Society of London.

I.—Dec. 23, 1891.—W. H. Hudleston, Esq., M.A., F.R.S., Vice-President, in the Chair.—The following communications were read:

1. "On Part of the Pelvis of *Polacanthus*." By R. Lydekker, Esq., B.A., F.G.S.

The specimen described in this paper was acquired by the British Museum from the collection of the late Mr. Beckles, and is from the Wealden, probably of the Isle of Wight. It is the central part of a Dinosaurian ilium, with portions of sacral ribs attached.

The point of special interest is a flat plate of bone, evidently a portion of dermal armour, resting on the upper border of the ilium; and this suggests comparison of the specimen with the dorsal shield of *Polacanthus Foxii*. Such a comparison shows that the present specimen belonged to a Dinosaur closely allied to, if not identical with, *P. Foxii*.

2. "On the Gravels on the South of the Thames from Guildford to Newbury." By Horace W. Monckton, Esq., F.G.S.

The author stated that the greater part of the hill-gravel in the district referred to belonged to the Southern Drift of Prof. Prestwich, and that the valley-gravels for the most part consisted of material derived from the Southern Drift. Small patches of Westleton Shingle and Glacial Gravel occurred near Reading and Twyford.

He divided the Southern Drift into three classes:—

1. Upper Hale type, characterized by the abundance of small quartz pebbles and the scarcity of chert.

2. Chobham Ridges type, with abundance both of small quartz pebbles and chert.

3. Silchester type; quartz scarce, and chert very rare or altogether absent.

He described the localities at which these types occurred and their limits of distribution, and then referred to the Glacial Gravels of the Tilehurst plateau, which he believed to have been deposited before the excavation of the valley of the Thames between Reading and Goring.

The author then dealt with the valley-gravels, which he believed to be mainly derived from the hill-gravels of the immediate neighbourhood, and showed how the various types of hill-gravel had contributed materials for the valley-gravels. He explained that, with the possible exception of the Westleton Shingle, he entirely rejected the theory of marine action in connexion with the formation of these gravels, and thought that the Glacial Gravels were probably for the most part due to floods during melting of large quantities of ice. The remaining gravels, he believed, had been spread out by water in valleys; as denudation proceeded, the gravel, by protecting the ground upon which it lay, came to stand out as the capping of the plateaux and hills; as the gravel itself was denuded, the materials were carried to lower levels, forming new gravels; and

this process has been repeated up to the present time. He explained that Prof. Rupert Jones and Dr. Irving had already adopted this theory in part, but that he differed from them in the entire exclusion of marine action.

3. "The Bagshot Beds of Bagshot Heath." By Horace W. Monckton, Esq., F.G.S.

The author stated that certain changes in the classification of the Bagshot Beds had recently been proposed, and he gave reasons for preferring that at present in use, which was originally proposed by Prof. Prestwich in 1847, viz. a threefold division into Upper, Middle, and Lower Bagshot.

He then argued against the theory that the Upper and Middle Bagshot Beds overlap the Lower Bagshot on the north-western side of the Bagshot district, as had been suggested by Dr. A. Irving; and, dealing with the various localities where Upper Bagshot had been alleged to exist resting on Lower Bagshot or on London Clay, he contended that in every case the evidence in favour of Upper Bagshot age broke down on examination.

II.—Jan. 6, 1892.—W. H. Hudleston, Esq., M.A., F.R.S., Vice-President, in the Chair.—The following communications were read:

1. "On a new Form of *Agelacrinites* (*Lepidodiscus Milleri*, n. sp.) from the Lower Carboniferous Limestone of Cumberland." By G. Sharman, Esq., and E. T. Newton, Esq., F.G.S.

Among a large series of fossils obtained during the Geological Survey of Cumberland and Northumberland, there are two which are referable to that remarkable and rare group of Echinoderms, the Agelacrinitidæ. The more perfect of these specimens is from the Lower Carboniferous rocks near Waterhead, on the River Irthing, and forms the subject of this communication. The disc-like fossil is only about four-tenths of an inch in diameter, and scarcely rises above the shell to which it is attached; nevertheless, it is so well preserved as to allow much of its structure to be studied. It is referred to the genus *Lepidodiscus*, and is seemingly closely related to *L. Lebouri*, described by Mr. Percy Sladen before this Society in 1879; but it also has affinities with *L. cincinnatiensis* and *L. squamosus*. From all these, however, the present specimen differs in having the pyramid in the middle of the interradial space, in possessing shorter arms, and in being much smaller. This fossil is to be named *Lepidodiscus Milleri*, after Mr. Hugh Miller, under whose direction these fossils were collected by Mr. J. Rhodes.

2. "The Geology of Barbados.—Part II. The Oceanic Deposits." By A. J. Jukes-Browne, Esq., B.A., F.G.S., and Prof. J. B. Harrison, M.A., F.G.S.

The Oceanic deposits rest unconformably on the Scotland Series, with which they contrast strongly in every respect. They are divisible into five portions:—

1. Gray and buff calcareous marls (Foraminiferal).
2. Fine-grained red and yellow argillaceous earths.
3. Pulverulent chalky earths (Foraminiferal).

4. Siliceous earths (Radiolarian).
5. Calcareo-siliceous and chalky earths (Foraminiferal).

The whole series is more calcareous in the northern than in the southern part of the island, and layers of volcanic dust occur in it at various horizons. There is everywhere a passage from the more siliceous to the more calcareous earths.

From the palæontological and lithological evidence the authors conclude that the depth of water in which the Oceanic beds were deposited varied between 1000 and 2500 fathoms. The microscopical and chemical evidence shows that the Radiolarian earths are similar to modern Radiolarian ooze; that the calcareo-siliceous earths are similar to what is called by Prof. Haeckel "mixed Radiolarian ooze"; that some of the Foraminiferal earths are comparable to *Globigerina*-ooze from 1000 fathoms, and that others greatly resemble European Chalk; and, finally, that the coloured clays bear a strong resemblance to the so-called "red clays" of modern oceanic areas. Hence the raised Oceanic deposits of Barbados seem to present us with an epitome of the various kinds of deposits which are found on floors of warm seas at the present day. Equivalent deposits are known in Trinidad and Jamaica; and it is inferred by the authors that the whole Central American and Caribbean region was deeply submerged during the Pliocene period, leaving free communication at that time between the Atlantic and Pacific Oceans.

An Appendix by Mr. W. Hill treats of the minute structure of the Oceanic earths and limestones and of the Foraminiferal muds and detritial earths; and this is supplemented by a Report from Miss Raisin on the inorganic material of certain Barbados rocks.

3. "*Archæopneustes abruptus*, a new Genus and Species of Echinoid from the Oceanic Series in Barbados." By J. W. Gregory, Esq., B.Sc., F.G.S.

This genus belongs to a group of Echinoidea which has given some trouble to systematists, owing to the union of the characters of the orders Cassiduloidea and Spatangoidea; the other genera belonging to the group are *Asterostoma*, *Pseudasterostoma*, and *Palæopneustes*. The evidence of the new Echinoid throws light upon the affinities of these genera. The main points suggested by a study of the new species are:—(1) the abandonment of the name *Pseudasterostoma* as a synonym of *Palæopneustes*; and (2) the inclusion of the true *Asterostoma*, *Palæopneustes*, and *Archæopneustes* in the Adete Spatangoidea, whereby the Plesiospatangidæ are left as a more homogeneous family, though bereft of the chief interest assigned to it.

A tabular summary of the nomenclature of the group is given.

The best-known fossil species of *Asterostoma* and *Palæopneustes* occur in Cuba, in deposits referred to the Cretaceous owing to the resemblance of these Echinoids to the common Chalk *Echinocorys scutatus*. The new genus includes a species from the same deposit, which is probably of the same age as the Bissex Hill rock from which the new species was obtained; this is at the top of the Oceanic Series, and belongs to the close of the great subsidence.

CORRESPONDENCE.

CRYSTALLINE SCHISTS OF THE LEPONTINE ALPS.

Sir,—Permit me to express my sincere regret to Dr. Stapff for having abbreviated not only his name but also—what is worse—his life. How the second misconception arose I cannot tell, but it is certainly not a recent one. Perhaps I ought also to apologize for not referring to his papers more frequently, but the truth is that I have only seen one of them, and that (for reasons on which it is needless to enter) I had but little opportunity of consulting. For this neglect some of my fellow-workers will probably visit me with censure. Be it so, I can only say that I do not always find myself quoted "over the water," and in this matter take as my maxim: *hanc veniam petimusque damusque vicissim.*

Except for this, my only purpose in writing, is to excuse myself from discussing at present Dr. Stapff's friendly and interesting communication. I am still at work on the subject of that singular complex of rocks in and about the Urserenthal, and cannot publish anything more till I have tested certain hypotheses on the ground. This I fear cannot be done during the present summer, since I anticipate that my steps must be turned in another direction, and I am not one of those fortunate persons who can undertake a long journey at pleasure in order to investigate a geological problem.

So I ask permission only to observe:—

(1). That I do not deny the possibility of Jurassic rocks or Carboniferous rocks entering into the complex of the Urserenthal. But I doubt the occurrence of organisms in the Altkirche marble. Without seeing the slides, it would be difficult to express an opinion on the nature of the objects figured by Dr. Stapff on page 18 of this volume; the upper one certainly has an organic aspect; the lower strikes me as more doubtful. But the nature of the objects is not the only thing to be considered.

(2.) That, if I am right in understanding Dr. Stapff to assign the Piora schists to the Carboniferous system, this identification appears to me only an hypothesis. If there be any valid evidence in favour of it, this is unknown to me, while I am aware of some serious difficulties in which we should be landed by accepting it.

(3.) That, from what I know of crystalline rocks and their ways, I venture to doubt the accuracy of the identification (p. 17) of "rolled quartz grains (*sand*) in some beds of the Guspis micaceous gneiss." For years I hunted for traces of an original clastic structure in gneisses and certain associated crystalline schists, longing to find them, but in vain. Again and again I have seen them curiously simulated here and there, by the results of pressure, and so, having been often taken in for a while, I have become rather sceptical.

T. G. Bonney.

CONCHOLOGICAL NOMENCLATURE.

Sir,—Mr. A. J. Jukes-Browne, in the January Number of the Geological Magazine, takes objection to some points in Conchological Nomenclature adopted in the "Systematic List of the F. E.

Edwards Collection of British Oligocene and Eocene Mollusca," to which I beg to offer the following remarks.

Mr. Jukes-Browne calls attention to the proposed disuse of *Cytherea* and *Triton*; two generic names which the reviewer discussed when noticing my book in "Nature" of October 29th, 1891. In a subsequent issue of the same Journal (November 12th, 1891), Baron Osten Sacken advocated the retention of *Cytherea* because the earlier Dipteroid genus of the same name being a synonym, and therefore rendered obsolete, could, from his point of view, be retained for another group.

Evidently these writers have not consulted the literature dealing with the Molluscan genera under discussion, or they would have ascertained that Lamarck's *Cytherea* had been replaced by his earlier *Meretrix* by many competent authorities such as Dr. J. E. Gray in 1847 (Proc. Zool. Soc. p. 183), Deshayes in 1853 (Cat. Conchifera British Museum, p. 34), H. and A. Adams in 1857 (Genera, p. 423), and other specialists, including Dr. Paul Fischer, who, in the latest and most elaborate treatise (Manuel, 1887, p. 1079) on the Mollusca, fully adopts it.

Concerning the name of *Triton*, we find that it has been used for three separate organisms: by Linnæus for a Cirripede in 1767; for an Amphibian by Laurenti in 1768; and for a Mollusk by De Montfort in 1810.

Writers on the Reptilia have ceased to regard it as one of their genera, because the Linnæan name has priority, and they have substituted *Molge* for it, a genus founded by Merrem in 1820. On the same grounds Malacologists also refuse to acknowledge it (as exemplified by the works of H. and A. Adams, Philippi, Weinkauff, Stoliczka, Zittel, Dall, etc.). Link's *Tritonium* of 1807 being the name now generally known for this shell, but as this differs from Müller's *Tritonium* of 1776, I have utilized the next most appropriate synonym, and brought into prominence Schumacher's *Lampusia* of 1817.

I hope this explanation will serve to show Mr. Jukes-Browne and others interested in this subject that the rejection of *Cytherea* and *Triton* as generic names in Zoology, being brought about through the operation of the law of priority, is now almost universally acknowledged.

R. Bullen Newton.

British Museum (Natural History), Cromwell Road,
January 13th, 1892.

READE'S THEORY OF MOUNTAIN BUILDING.

Sir,—I read Mr. Jukes-Browne's criticisms of some points in my "Origin of Mountain Ranges"[1] with interest, and until I came to the Postscript, which, like a lady's letter, contains the most important part of the communication, contemplated replying to them. This last paragraph however being destructive of the need of the preceding criticisms puts another complexion on the matter.

Mr. Jukes-Browne must be aware that I have replied to Mr. Davison's arguments against the "expansion theory of Mountain

[1] Geol. Mag. Jan. 1892, p. 24.

evolution,"[1] and it appears unreasonable to expect me to discuss a fundamental principle at second hand, especially with the inadequate materials contained in the Postscript. Until Mr. Jukes-Browne has brought this central idea of Mr. Davison's, which he adopts, into harmony with his own ideas, it would be a waste of my time to traverse his criticisms, some of which present themselves to my mind as exceedingly immature. When this is done I shall be prepared to consider his arguments, and I must also ask him to be good enough to restate the first paragraph on page 28, as after re-reading I fail to understand it. His quantitative illustration is unfortunate as he has only exacted a tithe of what he is entitled to in my figures:— $500 \times 500 \times 20$ is not five hundred thousand, but five millions.

PARK CORNER, BLUNDELLSANDS, *Jan.* 8, 1892. T. MELLARD READE.

OBITUARY.

HERR GEHEIMER BERGRATH PROFESSOR DR. C. FERDINAND VON ROEMER,

FOREIGN MEMBER. GEOLOGICAL SOCIETY, LONDON.

BORN 5 JAN. 1818. DIED 14 DEC. 1891.

C. FERDINAND VON RÖMER was born at Hildersheim, in Hannover, in which kingdom his family occupied a position of some distinction, his father being a Councillor of the High Court of Justice, and his elder brother, Frederick Adolph, being a geologist of repute. Until the age of 18, Ferdinand Römer lived at Hildersheim and received his early education in the Evangelical Gymnasium of that town. In 1836 he removed to Göttingen, where he studied for four years, with the exception of a break of six months at Heidelberg: he had been enrolled as a student of the Faculty of Jurisprudence, but began to attend lectures in natural science, and soon became so interested in this subject as to entirely abandon his legal studies. In 1840 he proceeded to Berlin, and in 1842 the University of that city conferred upon him the degree of Ph.D. in appreciation of a palæontological thesis, "De Astartarum genere." Dr. Römer remained here for another three years, devoting his vacations to investigations on the older rocks of Western Germany. His main results upon this subject were published in 1844 in "Das rheinische Uebergangsgebirge." In the spring of 1845 he sailed for America; he made a very extensive tour through the States, and devoted a year and a half to the study of the geology of Texas, and especially of the Palæozoic and Cretaceous rocks of the western part of that State. He returned to Europe in November, 1847, and settled at Bonn, where he lived till 1855 as a "privat-docent," but occupied mainly in the elaboration and publication of the results of his American expedition. The most important of these was his "Die Kreidebildungen von Texas" (1852), which, with some smaller papers, have been recently described by Prof. Dumble,[2] the chief of

[1] GEOL. MAG. June, 1891, p. 272.

[2] E. T. Dumble, Geol. Surv. Texas, Rep. State Geol. 1889, p. xxii. Austin, 1890.

the new Texan Geological Survey, as affording "a remarkably comprehensive view of the geology of the State."

In 1855 Römer accepted the Chair of Geology, Palæontology and Mineralogy in the University of Breslau; thenceforth his strictly geological work was mainly devoted to Silesia, his chief results being included in his "Geologie von Oberschlesien" issued as three quarto volumes in 1870. For this work he was knighted and appointed Geheim Bergrath of Silesia. But during the whole of this period he did not rest in peace at home: his travelling instincts, doubtless stimulated by his American experiences, repeatedly drove him to wider fields: thus in addition to tours in England, Belgium, Poland and Austria, he visited Sweden (1856); Norway (1859); Russia (1861); Turkey (1863); and Spain (1864 and 1871). In 1859 he was elected a Foreign Member of the Geological Society, by which he was also awarded the Murchison Medal in 1885. The later years of his life were spent at Breslau, busily engaged until the end, which came with sad suddenness just before the attainment of the jubilee of his doctorate; this his many friends and students were preparing to celebrate, out of respect for his high character and personal popularity, and in gratitude to his power as a teacher.

On turning to Ferdinand von Römer's work in science, one cannot but be impressed with his wide range of interests and knowledge: it seems doubtful whether he will be longest remembered as a geologist or palæontologist. In the former department he has added greatly to the knowledge of the stratigraphy of America and the countries that he visited; he worked at one time or another on nearly every system from the early Palæozoic to the Pleistocene; but probably his work on the Devonian rocks was the most important, ranging as it did right across Europe, from Devonshire to Constantinople. His palæontological works were very numerous and included papers on the Sponges, Graptolites, Rugosa, Ostracoda, Eurypterida, Arachnida, Bryozoa, Lamellibranchiata, Cephalopoda, all classes of Echinodermata; the Ophidia and Mammalia. Many of the genera he added to science were of exceptional interest, such as *Stephanocrinus* and *Dorycrinus*, while his discovery of the pinnules in Blastoids, and his work on the anatomy of *Cupressocrinus*, and the structure of *Melonites*, were important contributions to morphology. His monographs on the Asteroidea and Crinoidea of Bundenbach, on the Blastoids, and on the fauna of the Bone Caves of Ojcow, in Poland (of which an English translation was issued in 1884), and his "Die Fauna der silurischen Diluvialgeschiebe von Sadowitz," were all valuable additions to palæontological literature. His "Lethæa Palæozoica" issued between 1876 and 1880 as the first part of the third edition of the "Lethæa Geognostica" (with the early editions of which he had been associated) was a work of vast labour and permanent value. In later years Römer also wrote on mineralogy, issuing papers on the zinc ores, scheelite, columbite, and the pseudomorphism of cerrusite after cotunnite. But these three subjects did not exhaust his range of interests, for he was well read in literature both modern and classical, and his "Texas, mit besonderer

Rücksicht auf deutsche Auswanderung," showed his keen sympathy with the political and social problems of the time. J. W. G.

[As a friend and companion, Dr. Ferdinand Roemer was one of the most cheerful and congenial of men; the Editor is reminded of a delightful fortnight spent in his society in the Eifel, in 1878. His spirits and fun never seemed to become exhausted, and his vast stores of scientific knowledge were always at the disposal of his companions. His memory will be cherished by a large circle of younger men to whom his unvarying kindness will always be recalled with a sense of pleasing regret.—H. W.]

MISCELLANEOUS.

A Monument to William Smith, LL.D.

One of the most interesting historical collections preserved in the British Museum (Natural History) is the Geological Collection of William Smith, LL.D. This was commenced about the year 1787, and purchased by the Trustees in 1816, a supplemental Collection being added by Dr. Smith in 1818.

It is remarkable as the first attempt made to identify the various strata forming the solid crust of England and Wales by means of their fossil remains. There had been other and earlier Collections of fossils, but to William Smith is due the credit of being the first to show that each bed of Chalk or Sandstone, Limestone or Clay, is marked by its own special organisms, and that these can be relied upon as characteristic of such stratum, wherever it is met with, over very wide areas of country.

The fossils contained in this Cabinet were gathered together by William Smith in his journeys over all parts of England during thirty years, whilst occupied in his business as a Land Surveyor and Engineer, and were used to illustrate his works, "Strata Identified by Organized Fossils," with coloured plates (quarto, 1816; four parts only published); and his "Stratigraphical System of Organized Fossils" (quarto, 1817).

A coloured copy of his large Map, the first Geological Map of England and Wales, with a part of Scotland, commenced in 1812, and published in 1815—size 8 feet 9 inches by 6 feet 2 inches, engraved by John Cary—is exhibited in the Geological Department near his collection.

William Smith was born at Churchill, a village of Oxfordshire, in 1769; he was the son of a small farmer and mechanic, but his father died when he was only eight years old, leaving him to the care of his uncle, who acted as his guardian. William's uncle did not approve of the boy's habit of collecting stones ("pundibs"= *Terebratulæ*, and "quoit-stones"=*Clypeus sinuatus*); but seeing that his nephew was studious, he gave him a little money to buy books. By means of these he taught himself the rudiments of geometry and land-surveying, and at the age of eighteen he obtained employment as a land surveyor in Oxfordshire, Gloucestershire, and other

parts, and had already begun carefully and systematically to collect fossils and to observe the structure of the rocks. In 1793 he was appointed to survey the course of the intended Somersetshire Coal-Canal, near Bath. For six years he was the resident engineer of the canal, and, applying his previously-acquired knowledge, he was enabled to prove that the strata from the New Red Marl (Trias) upwards followed each other in a regular and orderly succession, each bed being marked by its own characteristic fossils, and having a general tendency or "dip" to the south-east.

To verify his theory he travelled in subsequent years over the greater part of England and Wales, and made careful observations of the geological succession of the rocks, proving also, by the fossils obtained, the identity of the strata over very wide areas along their outcrops.

His knowledge of fossils advanced even further, for he discovered that those *in sitû* retained their sharpness, whereas the same specimens derived from the drifts or gravel-deposits were usually rounded and water-worn, and had reached their present site by subsequent erosion of the parent-rock.

In 1799 William Smith circulated in MS. the order of succession of the strata and imbedded organic remains found in the vicinity of Bath.

His Geological Map of England and Wales is dated 1815.

On June 1, 1816, he published his "Strata Identified by Organized Fossils," with illustrations of the most characteristic specimens in each stratum (4to.).

In 1817 he printed "A Stratigraphical System of Organized Fossils," compiled from the original geological collection deposited in the British Museum (4to.).

In 1819 he published a reduction of his great Geological Map, together with several sections across England.

These sections have lately been presented to the British Museum by Wm. Topley, Esq., F.R.S., F.G.S., and are exhibited upon the wall near Smith's bust in the Geological Gallery (No. 11), see Guide-Book.

Mr. Smith received the award of the *first* Wollaston Medal and Fund in 1831, from the hands of Prof. Sedgwick, the President of the Geological Society—"As a great original discoverer in English geology, and especially for his having been the first, in this country, to discover and teach the identification of strata, and to determine their succession by means of their imbedded fossils."

In June, 1832, the Government of H.M. King William the Fourth awarded Mr. Smith a pension of £100 a year, but he only enjoyed it for seven years, as he died 28 August, 1839.

In 1835 the degree of LL.D. was conferred upon Mr. Smith by the Provost and Fellows of Trinity College, Dublin.

The highest compliment paid him was that by Sedgwick, who rightly named him "the Father of English Geology."

The bust above the case which contains William Smith's collection is a copy of that by Chantry surmounting the tablet to his memory in the beautiful antique church of All Saints, at Northampton, where his remains lie buried.

A monument has just been erected by the Earl of Ducie, F.R.S., F.G.S., to the memory of William Smith, at Churchill, Oxfordshire, where he was born; a village already famous as the birthplace of Warren Hastings.

The monument is formed of huge Oolitic ragstones of the district, similar to the Rollright stones. The name "Oolite" was given by William Smith to the rocks of the formation of which the higher grounds in this locality are a part.

This view of William Smith's Monument at Churchill has been prepared from a photograph taken by Lord Moreton, to whom we are indebted for permission to reproduce it here.

It is a monolith standing on a double base. The lower base is $10\frac{1}{2}$ feet square, and $3\frac{1}{4}$ feet high, the upper one is $6\frac{1}{4}$ feet square, and $2\frac{1}{2}$ feet high. The monolith stands 9 feet high above the upper base, and is about 3 feet square. A marble slab is inserted in the side facing the road from Chipping Norton, and bears this inscription:—"In Memory of William Smith, 'The Father of British Geology'; Born at Churchill, March 23rd, 1769; Died at Northampton, August 28th, 1839. Erected by the Earl of Ducie, 1891."